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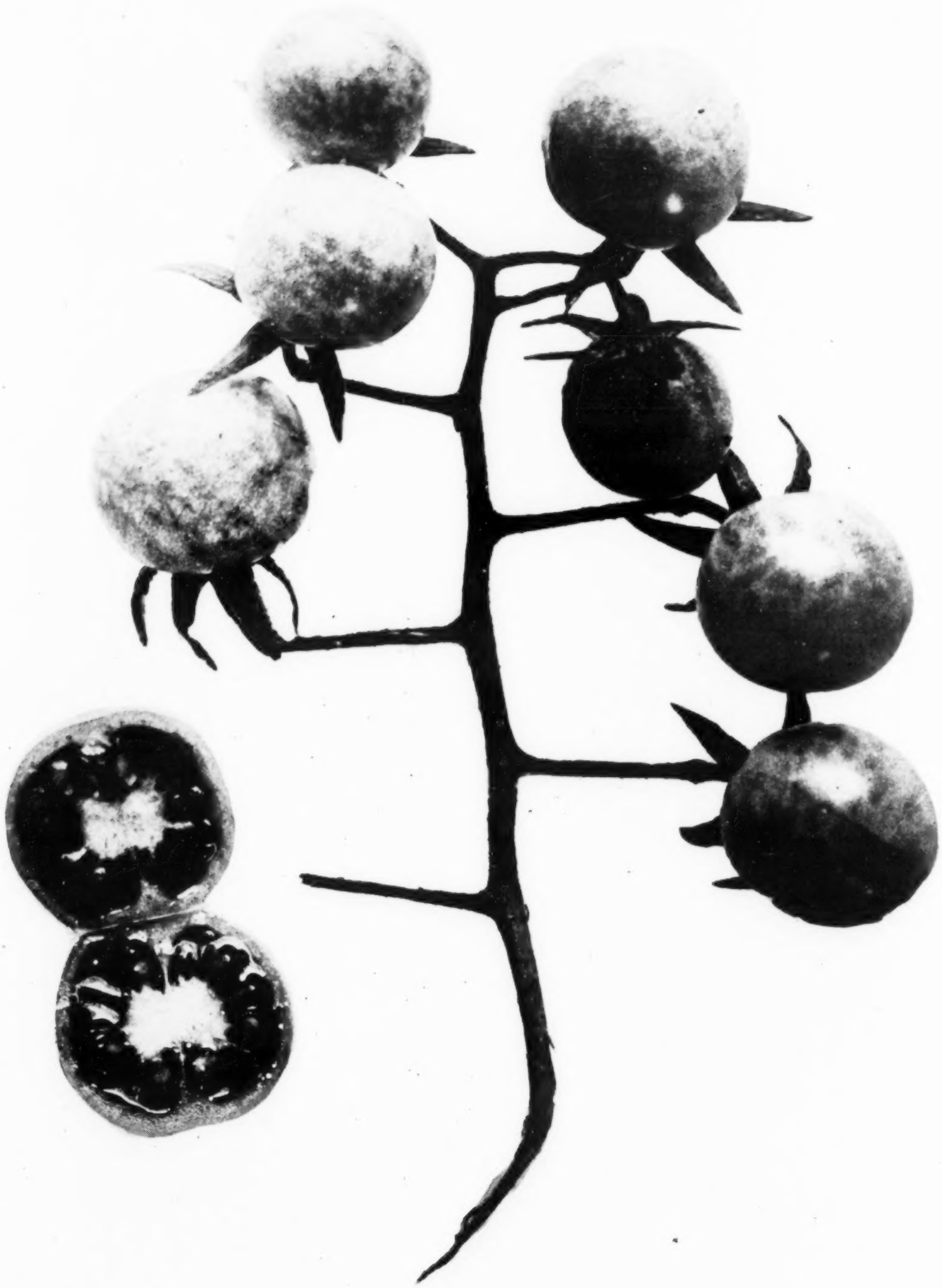
Date of issue of this number, May 25, 1915.

THE WILD TOMATO



The little wild tomato (*Lycopersicum vulgare cerasiforme*) which is found in Madeira is generally considered by botanists to belong to the original stock, native in South America, from which our cultivated varieties have been derived. It grows wild in many parts of the islands, often under desert conditions, and in appearance the plant is quite similar to the garden varieties. One plant was found where it could not have had a drop of water for at least three months. It probably had started to grow during the last few rains of spring, but had completed its growth during the heat and drought of summer. When it was found, the vine was apparently dead and lying flat on the ground; the leaves had dried up and dropped off; but more than 300 fruits, all plump and firm, were clinging to the vine. The fruits are so very acid that they can be used for little else besides soups, and the natives do not use them a great deal for even that. Their keeping quality, however, might prove a desirable characteristic in crossing with some of the highly developed varieties with the object of obtaining a good shipping tomato of pleasing flavor. Photograph, actual size.

(Frontispiece)

CHARLES H. GABLE, *Funchal, Madeira.*

POLLEN STERILITY IN GRAPES

Some Varieties Self-sterile—Study of Pollen Shows That It Becomes Worthless through Degeneration of Cell-nucleus—Remedy in the Planting of a Mixture of Varieties in the Vineyard.

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A NUMBER of varieties of grapes fail to set fruit when pollinated with their own pollen. This fact has been observed by grape growers for some time, especially when large blocks of certain varieties have been grown in more or less isolated positions. The reason for this has been given careful study by Beach ('98 and '99) and Booth ('02) working at the New York State Experiment Station. Munson ('09) in Texas and Whitten ('99) in Missouri have also contributed much in this work. Later, working in North Carolina with some of the southern grapes, Reimer and Detjen ('10) showed that a similar condition with respect to sterility existed in a number of these varieties. These have been the important attempts to determine which varieties will fruit when standing alone.

Testing Sterile Varieties: A brief statement, setting forth the main facts developed in their work will be interesting in this connection. Tests were made of the ability of a large number of varieties to set fruit when pollinated with their own pollen, by enclosing clusters in paper bags before the blossoms opened. In those varieties which are self sterile, pollen is produced in the usual quantities so that the failure to produce pollen is eliminated as a reason for sterility. An interesting fact developed in this bagging work was that pollen from those varieties which failed to set fruit when self pollinated also failed as an effective pollenizer when used on other varieties. These facts show that sterility in the grape is due to a lack of functioning in the pollen rather than in the pistil.

To illustrate from the work of Beach, when 143 clusters of Brighton were covered with bags and self pollinated, the average rating of the clusters formed, counting 100 as a perfect cluster, was approximately one, and, when thirty-two clusters distributed among 8 other varieties were pollinated with Brighton pollen, the average rating was three, showing Brighton, for those varieties used, as well as for itself, to be a poor pollenizer. On the other hand, when 116 clusters of Catawba were selfed, the average rating on the same basis as above was eighty-six, as compared with one in Brighton. When thirty-three clusters of eight other varieties were pollinated with pollen from Catawba, the average rating was sixty-seven, showing a marked difference between the Brighton pollen and the Catawba pollen when used either in selfing or crossing.

EXPERIMENTAL EVIDENCE

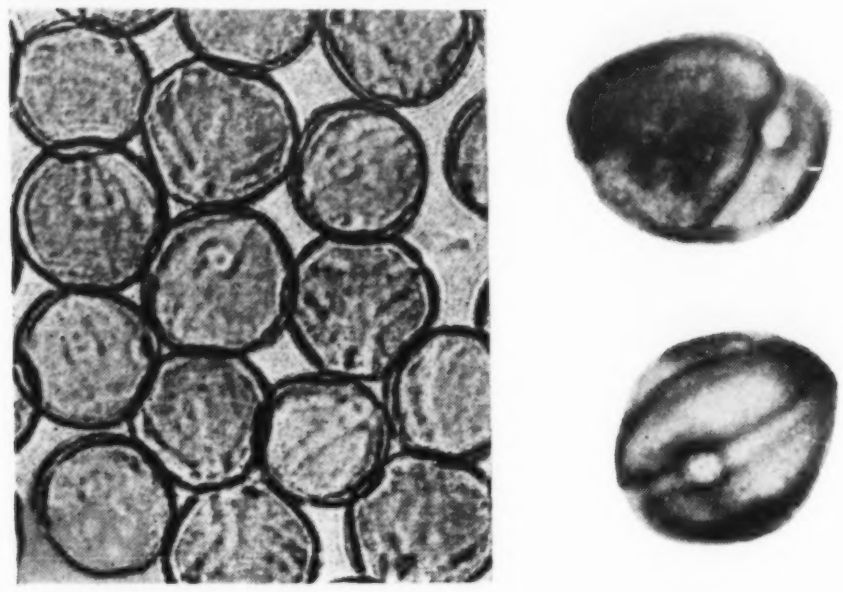
Beach worked with a large number of varieties and by means of the bagging method classified them with respect to their ability to fertilize themselves and also as cross pollenizers. Figs. 1 and 2 show two mature clusters of Brighton, illustrating the result when this variety is self-pollinated and when it is fecundated with the pollen of some other variety.

Reimer and Detjen ('10) extended this information and showed a similar condition to exist in a number of varieties of *Vitis rotundifolia* grown in the South.

Booth ('02) showed that the pollen from self sterile varieties was markedly different, when dusted upon a slide and examined with a microscope, from the

THE PROBLEM OF GRAPE POLLINATION

In many vineyards a poor crop of grapes is obtained, because the flowers are not properly pollinated. Many varieties are self-sterile, and must be pollinated with pollen from the flowers of some other variety; if there is no other variety bearing potent pollen near at hand, this does not take place, and the crop is a failure, as graphically shown in these two photographs. The one above shows a bunch of Brighton, a self-sterile variety, which has been pollinated only from its own pollen; on the opposite page is a bunch of Brighton cross-pollinated from another variety. Photographs from U. P. Hedrick, Geneva, N. Y. (Figs. 1, 2.)



NORMAL GRAINS OF GRAPE POLLEN

At the left, photomicrograph of a quantity of potent pollen grains from the variety Clinton, mounted in lactic acid; at the right, external view of the suture and germ-pore of fertile pollen from a staminate vine grape of the wild-river bank (*Vitis vulpina*). Each pollen grain, when dry has three longitudinal folds or sutures in the thick outer covering and when it is put in water these unfold and the grains often become twice as large as when dry. (Fig. 3).

normal fertile pollen. The normal pollen is oblong in outline with slightly flattened ends when dry, while the sterile is quite irregular and folded. By careful experiments he showed that the irregular grains, whether occurring mixed with the normal grains or not, failed to germinate when placed in a nutrient sugar solution. As a result of this work it is possible to tell whether a new variety is self sterile by means of a microscopic examination of its pollen.

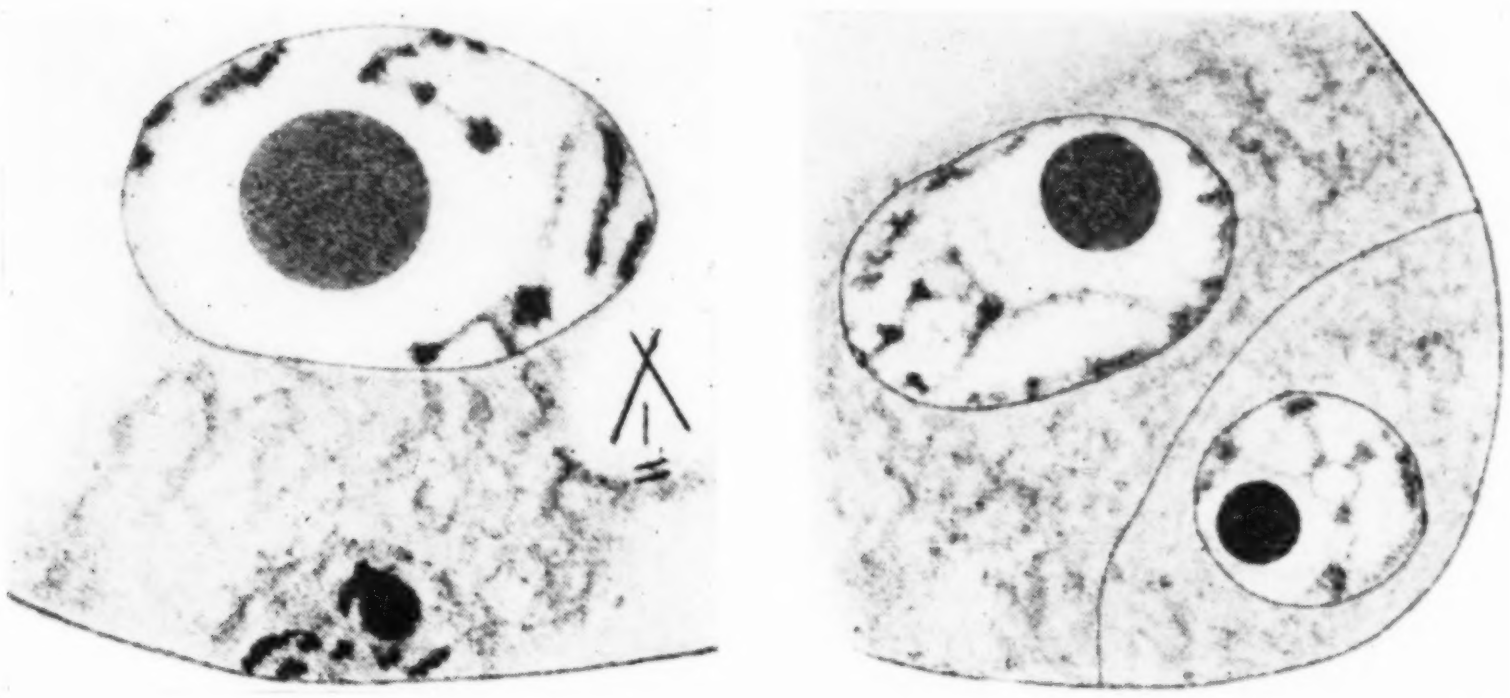
Flower Types: As is well known, there occur in the grape three types of flowers, (1) those with stamens upright and pistils abortive (this type of flower is functionally staminate), (2) those having upright stamens and pistils fully developed and functional (the perfect flowers), and (3) those with reflexed stamens containing sterile pollen and fully developed pistils (functionally the pistillate flowers). While the cultivated varieties quite generally produce functional pistils, there are, however, occasional vines found which have only a partial development of the pistil. The writer ('12) showed that, with respect to pistil development, practically a complete series of intermediates occur between those forms which are classified as staminate and those classified as pistillate.

A classification based upon stamen type, of 132 important commercial varieties, shows that there are ninety-five varieties with upright and thirty-seven with reflexed stamens. Of the ninety-five only eleven are classed as self sterile or partly so, while only two of the thirty-seven having reflexed stamens were partly fertile, the remainder being sterile.

SUMMARY OF RESULTS

These results may be briefly summarized as follows: (1) Self sterility in the grape is due to the pollen. (2) All varieties tested set fruit when potent pollen was used, which shows that the pistils are normal. (3) Certain varieties are more effective as pollenizers than others. (4) When dry, potent pollen can be distinguished from impotent by its shape. (5) Impotent pollen is correlated with the reflexed type of stamen.

The Nature of Sterile Pollen in the Grape: It will be interesting, now, since it has been shown that the pollen borne by a number of American varieties of grapes is more or less impotent, to



WHY SOME POLLEN IS WORTHLESS

Careful study has shown that in most instances the inefficacy of pollen is due to the fact that the generative nucleus in each cell has degenerated. At the right is a camera lucida drawing of part of a pollen grain of the variety Concord, showing both nuclei normal. The vegetative nucleus, at the top, plays its part in producing the pollen tube, after the pollen grain falls on the stigma and starts to germinate. Down this tube the nuclei formed by a division of the generative nucleus, in the lower part of the figure, will pass, one of which will unite with the egg-cell nucleus to begin the formation of the seed. At the left is shown part of a Brighton pollen grain, in which the vegetative nucleus is normal but the generative nucleus has degenerated. (Fig. 4.)

determine the nature of this impotency. This has been the subject of some investigations by the writer in the past few years and by careful examination of pollen development in both sterile and fertile forms some interesting facts have been developed. (Dorsey '14.)

These studies have been carried forward by means of the usual cytological technique and have involved a careful study of pollen development in both the sterile and fertile forms. Particular attention has been given to a study of (1) aborted pollen and (2) the degeneration of the nuclei in the mature pollen.

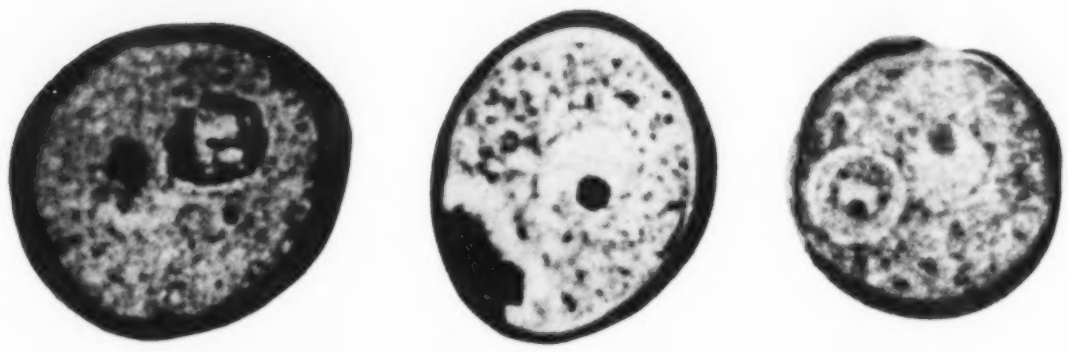
It might be well before taking up a consideration of these two topics to state briefly the stages in pollen development which take place normally. Early in the spring the tissues of the anther differentiate and produce pollen mother cells. These undergo further growth and differentiation, passing through stages well known in the higher plants. The pollen mother cell divides and then divides again, so that we have produced from each pollen mother cell four nuclei. From these, the so-called microspores are formed. These young microspores, or one-celled pollen grains, undergo a period of growth and enlargement and are finally separated; the nuclei of these pollen grains again divide, forming the mature pollen grain with two nuclei, the smaller one of which is the generative nucleus, which by another division forms the gametes.

POLLEN DEGENERATION

Up to the formation of the microspores, development apparently takes place normally in the self sterile varieties. We shall consider the aborted pollen and degeneration of the nuclei in the mature pollen.

The Aborted Pollen: An examination of the pollen in the mature anthers of a number of varieties, both cultivated and wild, showed that aborted pollen is produced in varying quantities. In some varieties, this amounted to as much as 69% of the pollen produced, while in others there were practically no aborted grains. The aborted pollen was found in both self sterile and self fertile varieties and was produced in practically equal quantities in the wild staminate and pistillate vines of *Vitis vulpina*.

No abortion of pollen grains has been noticed in the grape previous to the



DEGENERATION OF GENERATIVE NUCLEUS

(a) Pollen grain of Brighton, in cross-section, showing commencement of degeneration in the generative nucleus (at the left). (b) Grain of same variety, in which the generative nucleus has wholly degenerated, and the vegetative nucleus is still functional. (c) Normal pollen grain of the wild grape, *Vitis Vulpina*, shown for contrast. Both nuclei here are still functional. Photomicrographs. (Fig. 5.)

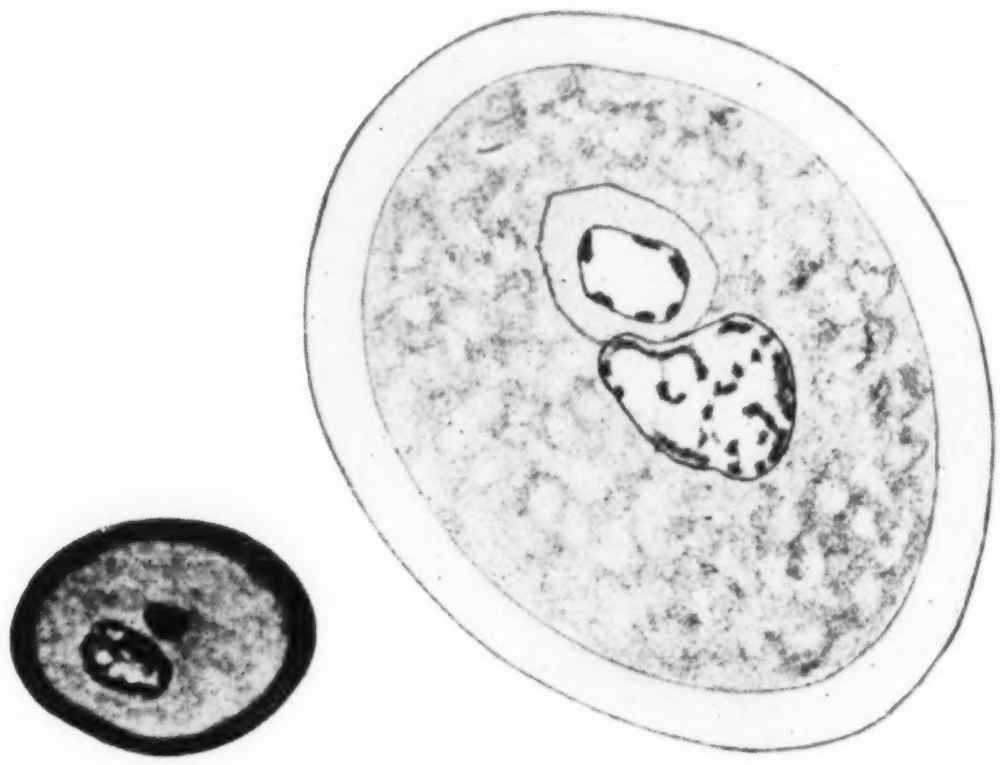
liberation of the microspores from the common mother cell. It is first noticeable during the early growth period of the free microspore and shows various degrees of arrested growth combined with loss of cytoplasm.

This aborted pollen, however, does not seem to be important from the standpoint of the setting of the fruit, since there is an abundance of pollen produced and even in the instance cited, where 69% of the pollen produced was of the aborted type, still there would be enough normal pollen, if potent, to insure a good setting of fruit.

The Degeneration of the Nuclei: In the mature pollen grain we have two nuclei, the generative and the vegetative. The generative nucleus, as has been stated, divides again to form the gametes, and the vegetative nucleus functions in the growth of the pollen tube. These two nuclei are shown in figure 4, the smaller one being the generative nucleus. A careful study of the pollen, produced by those varieties which bagging tests have shown to be more or less self-sterile, shows that the generative nucleus, and in some cases, also the vegetative, degenerate, as shown in figures 4, 5, and 6. Such degeneration precludes the possibility of normal functioning in any pollen where it occurs. These studies show that degeneration in both nuclei occurs in a large per cent. of the pollen in Brighton, while in other varieties which are self fertile, normal pollen (fig. 5c) is produced. Sterile pollen in the grape, then, is due to degeneration in the generative nucleus.

The Germ Pores: Another fact which is interesting in this connection is that the germ pores (Fig. 3) are not formed in pollen borne by the reflexed type of stamen. There is an interesting correlation, then, in the absence of the germ pore, sterile pollen, the reflexed type of stamen, and the tendency toward dioeciousness.

The general question of sterility in plants at the present time is being investigated from a number of standpoints. It has been known for a long time that a good many hybrids are sterile. Some workers, particularly Jeffries, have emphasized the relation between aborted pollen and hybridity. A number of cases are recorded where an unequal number of chromosomes are brought into the zygote from each parent. This is not the case, however, in such varieties as Brighton and Barry, which, like Concord and *V. vulpina*, have twenty chromosomes in the reduced number. From the heredity standpoint some workers hold that sterility results from the presence of a factor for sterility. The physiology of pollen growth and fertilization is now being investigated and it is probable that some interesting things will be discovered from this standpoint. All told, the influences leading to sterility are not well understood and we shall have to reserve judgment on a number of points until further work has been done.



DEGENERATION OF BOTH NUCLEI

In some cases both nuclei of the pollen grain degenerate. At the left is a photomicrograph of a pollen grain from Brighton, in which this has taken place; at the right is a camera lucida drawing much more enlarged of the same process, in another grain of the same variety. Note irregular shape of both nuclei. (Fig. 6.)

Finally, then, in the case of the grape, if sterility results from degeneration in the generative nucleus which prohibits normal functioning of the pollen, it is clear, that to overcome the defects of self sterility in any variety, it will be necessary to continue the practice firmly established by some of the earlier workers of mixing varieties in the vineyard.

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Great Men and How They Are Produced

In order to call attention to his offer which is now in the hands of the American Genetic Association, Casper L. Redfield (Monadnock Block, Chicago, Ill.) has issued a pamphlet with the title "Great Men and How They Are Produced." It gives partial genealogies for 571 eminent men, showing that in most cases such men are the product of slow breeding—that is, with an unusual amount of time between generations. Mr. Redfield has offered two prizes of $100 each to the American Genetic Association, if any one can bring forward instances of really great men produced by rapid breeding. The offer remains open until December 31, 1915, and its details can be learned from the JOURNAL OF HEREDITY, July, 1914, or from Mr. Redfield or this Association.

WELLESLEY'S BIRTH-RATE

Reproductivity of College Graduates Far from Adequate Even to Replace Their Own Numbers—Importance of the Problem and Suggestions for Its Solution.

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NO QUESTION is of greater importance to eugenics than that of the birth-rate among the eugenically superior parts of the population. The junior author has therefore been investigating the reproductivity of Wellesley College graduates; some of her data were presented by the senior author in his address on Marriage Selection before the Race Betterment Conference at Battle Creek, Michigan.[1] This investigation has now been completed, and the results are summed up in the following table:

WELLESLEY COLLEGE

Status in fall of 1912	Graduates	All Students
Per cent. married (graduated 1879-1888)	55%	60%
Per cent. married in:		
10 years from graduation	35%	37%
20 years from graduation	48%	49%
Number of children (mothers graduated 1879-1888):		
Per student	.86	.97
Per wife	1.56	1.62

From a racial standpoint, the significant marriage rate of any group of women is the percentage that have married before the end of the child-bearing period. Classes graduating later than 1888 are therefore not included in the first case, in which the status is of reports in the fall of 1912. In compiling this data deceased members and the few lost from record are of course omitted.

It is desirable to find any change that may be taking place in the marriage or birth rates, so we have calculated the rate at the end of ten and twenty years after graduation for each class. The twenty-year period so nearly covers the effectively fertile years of a woman's life that it is more significant than the unlimited rate of the '79-'88 classes. The result destroys the defense put forward by certain apologists for separate colleges, viz.; that the earlier college women were more professionally inclined, that their marriage rate was abnormally low for this reason, and that with the more varied classes of later years, the marriage rate must have risen. Let us hope there has been a change for the better in the uncharted last ten years: but there is nothing in the steady decline of the previous years to give any confident basis for such a hope.

In the address referred to above, statistics were given showing a lower reproductivity[2] of the honor girls (Phi Beta Kappa) resulting principally from a lower marriage rate. In order to test this further, we give the results of an

[1] JOURNAL OF HEREDITY, V, 3, pp. 102-110, March, 1914.

[2] The word *reproductivity* is used as a convenient term to give the net result as expressed in number of children per total number of women married or unmarried.

investigation of the honor girls before Phi Beta Kappa was established at Wellesley. These honors consisted of Durant and Wellesley scholarships, which carry no stipend and are therefore awarded by the faculty solely for excellence in studies. The previous findings in regard to Phi Beta Kappa girls are confirmed by this newer study, as follows:

WELLESLEY COLLEGE

GRADUATES OF '01, '02, '03, '04, STATUS OF FALL OF 1912

	All	Durant or Wellesley Scholars
Per cent. married	44	35
Number of children:		
Per graduate	.37	.20
Per wife	.87	.57

The extraordinary inadequacy of the reproductivity of these college graduates can hardly be taken too seriously. These women are in general, and from a eugenic point of view, clearly of superior quality, for

(a) They have survived the weeding-out process of grammar school and high school.

(b) They have survived the repeated elimination by examinations in college.

(c) They represent the number left, after those with lower mental abilities have grown tired of the mental strain and dropped out.

(d) Some have forced their way to college against obstacles, because seeking its mental activities, congenial to their natures.

(e) Some have gone to college because their excellence has been discovered by teachers or others who have strongly urged it.

All these attributes cannot be wholly mere acquisitions, but must be in some degree inherent. Furthermore, these girls are not only superior in themselves, but are ordinarily from superior parents, because

(a) Their parents have in most cases cooperated by desiring this mental training for their daughters.

(b) The parents have in most cases had sufficient economic efficiency to be able to afford a college course for their daughters.

Now, these select women, who should be having at least the 3.7 children each, which Sprague[3] calculates are necessary to maintain a stationary population, are only giving to the race .83 of a child each. Their reproductivity is only 22¼% of being adequate merely for replacement.

There are at least three causes for this abnormally low birt h-rate, viz.:

(1) Lack of coeducation.

(2) The failure of their education to make them desirous of having homes of their own and efficient in these homes.

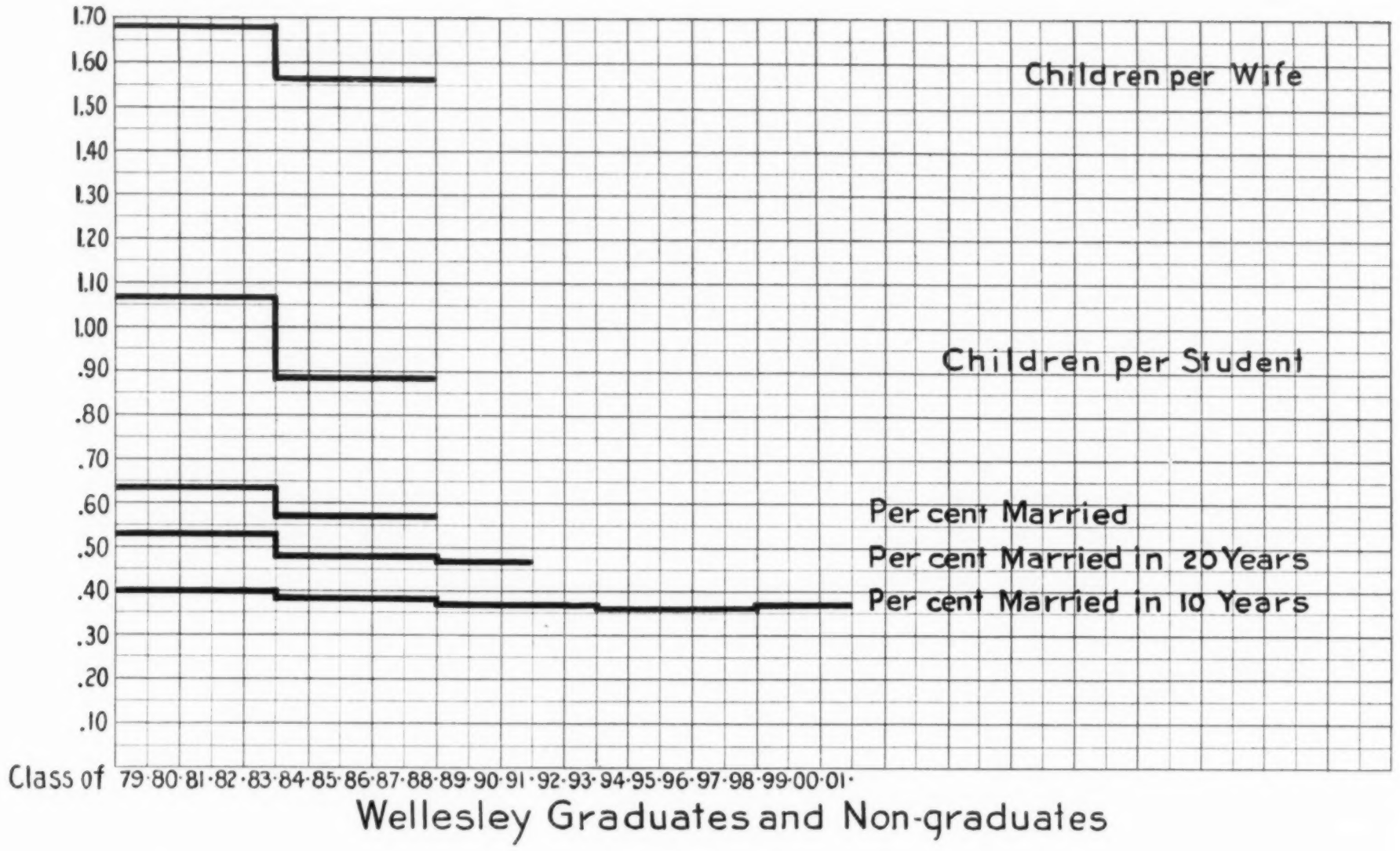
(3) Excessive limitation of the students' opportunities for social life.

Sprague expresses a doubt whether any adequate data in regard to the influence of coeducation on the marriage and birth rates have yet been collected. But we see no reason for rejecting the results of Miss Shinn's investigation (*Century Magazine*, October, 1895), desirable as further studies may be. She found that nearly 50% of the coeducational women married before the age of 30, but only 40% of the women from separate colleges. If one thinks this difference small, let him remember that even 1%, carried over a long period of time, would produce a great effect in a cumulative process such as evolution.

Furthermore co-education produces a larger percentage of marriages with college men.

Separate colleges for women, in the United States, are from the viewpoint

[3] Sprague, Robert J., Education and Race Suicide. JOURNAL OF HEREDITY (Organ of the American Genetic Association), Vol. VI, No. 4, pp. 158-162, Washington, D. C., April, 1915.



Graph showing at a glance the record of the student body in regard to marriage and birth rates, during the years indicated. Statistics for the latest years have not been compiled, because it is obvious that girls who graduated during the last fifteen years still have a chance to marry and become mothers. (Fig. 6.)

of the eugenist an historic blunder. They arose because (1) women were debarred from the eastern men's colleges —a most unfortunate circumstance, (2) because the mental capacity of women was at that time all too frequently considered to be too inferior for college training. It was, therefore, a natural result that colleges for women should be established. But, unfortunately, to correct the current depreciation of woman's mentality, it was thought necessary to give her the same curriculum as that used by men.

The results of the experiment, however, have been utterly inconclusive because no direct comparison of the men and women was possible. It was in the coeducational colleges that the test was conducted under satisfactory conditions. Today it is well known that the women capture more than their proportion of the honors and average higher in their marks. Is there any real reason, then, for these eastern, separate, women's colleges to continue along the same old lines, with the unsatisfactory results that we have seen?

The stubborn resistance of these colleges to the introduction of education for domestic efficiency, especially in the care of the infant, has been amazing. They are thereby neglecting one of the most important factors in a woman's sound education.

May it not be that this ill-adjusted education is partly responsible for the fact that Cattell finds in American men of science at the time of his inquiry that those having college graduates as wives had 2.02 children each, while those with wives of partial college training had 2.12 children and those with wives of no college education 2.35 children?

The very proper preference in many intelligent men for girls trained to be efficient wives and mothers is one of the causes of the low marriage rate and late time of marriage of the graduates of the women's colleges. The trained girl can and will marry a man with an income too restricted for the support of an inefficient wife.

Rules in force at various women's colleges, which lead to social limitations, not to say asceticism, throw up

barriers to the social opportunities of the students. And this during the critical years of maximum attractiveness when, as we have elsewhere shown, so many of the non-collegiate girls are marrying or making acquaintances leading to marriage. To take a specific instance: at Wellesley no young men are allowed to call on a student during her one free day, Sunday.

Since, then, the separation of sexes in different colleges, and the failure to teach girls domestic science, are contrary to the interests of society and the race, should we not urge:

(1) Parents to send their daughters to coeducational universities, or at least to semi-coeducational ones such as Harvard and Columbia where they will have some opportunity to meet superior young men.

(2) The state or private benefactor to provide all men's colleges with closely affiliated women's colleges and all women's colleges with closely affiliated men's colleges, and to provide all women's colleges with strong departments for the teaching of domestic science in the broadest sense of the term. In case of refusal of the institution to accept such provisions, discrimination in the distribution of funds might well be made in favor of the more soundly organized institutions.

Conformation of Cows and Milk Yield

Inquiry into the relation between conformation of cows and milk yield has lately been made by J. Reimers, an agricultural teacher at Wageningen, Holland, and his conclusions were summarized in the *Mitt. der Deut. Landw. Gesell.*, for April 26, 1913. The data investigated by him were taken from the Friesian Cattle Herd Book, and had reference to 300 animals, from 2¼ to 3 years old. With regard to the statistical methods adopted, it may be explained that he divided the animals with milk yields between 4,400 pounds and 8,800 pounds into five classes, and attempted to correlate the yields with the various features of the conformation. He also divided the animals into classes according to their body measurements, and attempted to correlate these with the milk yields.

The investigators' findings, when summarized, were, generally, to the effect that no relationship existed between conformation and milk yield. His conclusions were as follows:

The milk yield increases slightly with increasing *length of body* until the latter reaches a certain point, after which there appears to be a slight decrease in the yield. Abnormal length of body apparently has the effect of lowering the milk secretion.

The milk yield increases with increasing *height of crupper*; but the increase can by no means be called regular, and a strong connection between the two factors could not be established.

Animals with small or with very deep breasts appear to give a smaller yield than animals which are normal in this respect, but the difference was too slight to make the deduction of practical value.

There is no regular relationship between milk yield and *length of hindquarters*, or *width between haunches*, or *breadth of pelvis*. Animals with normal breadth of pelvis give more milk than those with larger or smaller breadth of pelvis, but the difference is not important.

The system followed by herd book inspectors, in awarding points for conformation, was also taken as a basis of comparison. No more success was met with in this direction, except, of course, in the case of points for udder, teats, milk veins, and similar indications of good milk yield. In the Friesian Herd Book as many as twelve points are awarded for shape of hindquarters, but no relation between this and the milk yield could be traced. Further, the best milkers had the worst thighs (although there was no regular connection between this and the yield).—Journal of Board of Agriculture (England).

DYNAMIC EVOLUTION

Redfield's Theory of Inheritance of Results of Training and Use Not Supported by Adequate Biological Evidence.

A Review by Raymond Pearl
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THAT "dynamic" should appear as an integral part of the title of a book by Mr. Redfield is altogether appropriate.[1] As a writer he is nothing if not forceful. He has (by right of training and experience) all of the good lawyer's skill in carrying the reader deftly across weak places in the logic of the argument or over lacunae in the factual basis, by means of a flow of vigorous and sometimes brilliant rhetoric. His writings have been followed with great interest by the present reviewer for ten years or more past. The occasion of that interest, however, has not been primarily genetics, but rather the psychology of scientific paradoxy. De Morgan's most delightful "Budget of Paradoxes" dealt with those ancient and honorable paradoxers who squared the circle, made machines calculated perpetually to move, and engaged in similar activities. In his day the biological paradoxer had not yet appeared in numbers, and the genetic paradoxer was totally absent. It would have delighted De Morgan beyond measure to have dissected Mr. Redfield's biology and through the maze of specious verbiage shown, at point after point, where that author's methods of scientific reasoning and proof deviated from those of his more orthodox colleagues.

As a scientific investigator Mr. Redfield labors under at least one very serious handicap. It is that he is firmly committed to a thesis in advance of the investigation. This assertion he would no doubt deny vigorously. I am entirely willing to rest my case as to its validity on the sum total of Mr. Redfield's writings on heredity and related topics.

It would be difficult to state briefly this thesis in terms which would be entirely acceptable (I fear) to its author. I shall therefore merely try to state my understanding of it in ordinary terminology. It is essentially this: that as any organ of the body, or the body as a whole, or the mind, is used or exercised there results, in some manner not made clear physiologically, an accumulation or storage of energy in the germ cells with respect to the organ or function exercised. This accumulation of energy (with respect to a particular character) is then supposed to be transmitted to the next generation, again by some process not made clear physiologically, but according to definite rules. These rules of transmission, while always definite, have been variously and sometimes curiously modified in the course of Mr. Redfield's writings upon the subject. The practical moral for the breeder, however, is in general, that in breeding for performance of any sort it is wise to use as breeders individuals which have been "developed" and hence have stored energy. Further, this will in general mean (though Mr. Redfield's latest codification of the rules of energy transmission by sex, etc., etc., makes a perfectly general statement of his precepts impossible in few words) that the parents of great performers are likely to be found amongst relatively aged animals.

The present book is essentially a condensed epitome of the author's earlier writings upon this subject which

[1] Dynamic Evolution. A Study of the Causes of Evolution and Degeneracy. By Casper L. Redfield. New York and London (G. P. Putnam's Sons), 1914. Pp. 210, price $1.50 net.

have been published in various journals devoted to live-stock or fancy breeding interests. He has applied his thesis to trotting horses, setter dogs, Holstein cattle, and human beings. The first fourth of the book is devoted to an exposition of the theory in general terms. The discussion is of a most fundamental character. It has as its only initial assumption existence of matter and energy. From that somewhat unpromising, or at least very raw, material there is developed in some fifty odd pages, by pure ratiocination, a full blown theory of heredity.[2] This theory consists essentially of the thesis which has already been outlined. The attempt to ground it on elementary principles of physics and pure logic need not detain us.

The rest of the book is nearer the ground. It consists in an application of the theory to selected facts in the breeding history of the animal forms mentioned in the preceding paragraph. Mr. Redfield apparently regards these chapters as an exposition of the results of different tests of the validity of the theory. In any sense which a trained scientist would regard as critical none of the material presented can be looked upon as furnishing a definite test of the theory.

It would require altogether too much space to discuss critically all the points of alleged fact and their interpretation brought out in this book. It will perhaps be more useful to point out wherein the methods of "investigation" followed in general by Mr. Redfield seem to be faulty. In the first place our author apparently has no conception of the meaning of random sampling and the errors connected therewith. Many of his comparisons are essentially statistical in their nature, but there is not a "probable error" in the book. Thus on p. 85 he tables the mean age of fifty-eight sires of 2:10 stallions in the following way:

Sires of 2:10 *Stallions*

45 Sires with records, average age	9.8 years
13 Sires without records, average age	13.6 years
58 Sires of Stallions, average age	10.71 years

He then shows that the mean age of 1,000 trotting horse sires in general (presumably a random sample, but this is not entirely clear) is 10.43 years. I leave these figures to the consideration of the biometrician, with only the additional quotation of Mr. Redfield's comment upon the table: "The striking thing about the above table is the fact that one group of sires falls below this (the general) average while the other group is considerably above it. As age means time, and time is a factor of energy and not of anything else, any variation, other than a mere accidental one, in the average age of a group of sires from the normal average, has reference solely to work performed by those sires before reproduction. Whether the present variation is due to accident or to the operation of a law of nature we may determine by examining the sires in detail and learning what relationship existed between the ages of the different ones and the work they performed."

To speak about laws of nature on such a basis of fact is, to put it quite mildly, a thoroughly optimistic piece of business.

CONTRADICTIONS DISREGARDED

In the second place all unconformable facts, and all ways of looking at the facts different from the author's way are totally disregarded. For example, in the discussion of Laverack's setters the results which that breeder got are interpreted as solely due to the fact that he hunted his dogs and bred them relatively late in life. There is not the slightest real evidence that either of these two things had any more than a very minor part, if any, in accounting

[2] Incidentally in the process there are chipped off some real gems of thought. I would especially recommend to the consideration of professional philosophers this last sentence on p. 3: "Causes are forces acting upon matter."

for his results, whereas all that we know from live-stock breeding history, and from experimental data, would indicate that the primary factor in Laverack's success was inbreeding. Whether or not this is true, in any event the whole course of genetic knowledge during the last fifteen years shows most clearly that it is not wise to insist on any one factor as the *exclusively* important one in interpreting natural evolution or breeding practice.

In the third place, the author is either grossly ignorant of the literature of biology and physiology or else feels impelled in his theorizing to soar above all paltry consideration of ascertained biological fact. There are few pages of the book which do not contain some statement, put in the form of a positive, dogmatic assertion, which either has no foundation in fact whatever because the subject has never been investigated, or is contrary to well-known data in the literature of biology. For example, to take a perfectly random chance, the place where the book falls open as I write these words is p. 145. In the middle of that page occurs this statement:

"The amount of butter fat produced by a cow depends upon two factors, one of which is the quantity of milk produced, and the other of which is the percentage of fat in the milk. Of these, *the percentage of fat is more highly variable than the quantity of milk* and is the more important factor in determining a cow's admission to the Advanced Registry."

For the portion of the statement in italics (mine) I know of no warrant in fact whatever. All the quantitative studies on the variability of milk which have ever been published, including those of Gavin, Pearson, and Vigor, show that quantity of milk is relatively (measured by coefficient of variation) more variable than per cent of fat, in the ratio nearly of 2 to 1.

On the inside front cover of each issue of this journal stands the following statement as to one of the objects of the American Genetic Association: "The Association constantly strives to further the cause of conservative, constructive science and to check the progress of fallacious and sensational pseudo-science." With the purpose of contributing in some small measure to the worthy object of checking the progress of pseudo-science this review has been written. Like all pseudo-science, Mr. Redfield's is a conglomerate mixture of the true, the false and the unknown. There undoubtedly *is* "something in it." There is, for example, an increasing body of evidence that there exists quite generally a correlation between birth order and certain other characters of the organism. In so far age of parents is to be considered as a factor to be regarded in an analysis of breeding results. But the methods by which it is sought to make it appear that this is a factor of overwhelmingly great importance, involving as they do all the discussion of the hereditary transmission of accumulated energy, do not form any part of what the reviewer understands as science.

Annual Meeting of A. G. A.

Plans are rapidly being completed for the annual meeting of the American Genetic Association at Berkeley, August 2–7. It promises to be one of the most successful yet held, in point of excellence of program. Members who may be going to the Pacific coast in the late summer are urged to arrange their itinerary so that they may be present at this meeting.

Eugenics Research Association

The Eugenics Research Association will hold its annual meeting at San Francisco August 2-5, in connection with the annual meeting of the American Genetic and other associations. Particulars can be obtained from the secretary, William F. Blades, Cold Spring Harbor, Long Island, N. Y.

THE MARRIAGE OF KIN

No Adequate Evidence That Any Evil Results from Consanguineous Matings, as Such, Although Where Both Stocks Are Weak the Offspring May Show Double Amount of Weakness.

EDWARD NETTLESHIP[1]
University of London, England

THE subject of marriage between blood-relations should, I think, engage the attention of all who are interested in problems bearing upon the improvement of the race; it is at any rate one upon which there has been, and perhaps still is, much diversity of opinion. Such differences of view are doubtless often based, on the one hand, upon the experiences of certain single families where serious defects or degeneracies have appeared in the offspring of consanguineous marriages, and on the other upon acquaintance with families in which nothing undesirable has followed the marraige of first cousins. Indeed those who object, from what we may call individual or single-family experience, would perhaps be surprised to find that the children of cousins sometimes showed a decided improvement upon their parents. In short I venture to think that the subject is one upon which we may well seek more knowledge and greater clearness of thought.

The fundamental questions are (1) whether the offspring of consanguineous parents display inferior or degenerate characters in larger proportion than do the offspring of unrelated parents? And (2), if such an effect can be shown, is the appearance of these undesirable characters attributable to something produced *de novo* by the union of parents related in blood, but who themselves contain no trace of such characters, either manifest or hidden? Or are the defects only a result of both parents being tainted, but not tainted badly enough to show?

The second question is not merely academic. For if consanguinity can produce something bad, good, or indifferent that had never occurred before in the genealogy then no cousin marriage is safe. But if it is only a case of inheritance from both parents, a tainted pair who have no community of blood will, so far as we know, be as likely to have undesirable offspring as if they were tainted cousins; whilst cousins who are free from taint will be expected to yield normal children.

It must be said at once that the data for answering the first question upon statistical grounds do not exist because no one up to the present time has been able to obtain sufficiently accurate returns of the relative numbers of consanguineous and unrelated marriages.

FREQUENCY OF OCCURRENCE

In 1862 a French writer, M. Boudin, came to the conclusion that close upon 1% of the marriages in France between the years 1853 and 1859 were between first cousins (counting in a few between uncle and niece or aunt and nephew.) And he considered that if second-cousin marriages had been included the total percentage would be 2% This conclusion was based upon the official records of more than twenty million marriages. It is obvious that such a return would err, if at all, on the negative side; especially in a Catholic country such as France was then. In the Roman Catholic Church marriage between near cousins is forbidden unless an indulgence be obtained by payment; and obviously the liability

[1] Dr. Nettleship died shortly after preparing this paper, which was published in the Eugenics Review, VI, 2, 130, London, July, 1914. It is here reprinted slightly abridged.

to such an exaction would sometimes lead to concealment of cousinship. Somewhat later returns (up to 1875) showed a rather higher proportion (1.5%) of first-cousin marriages, the consequence, apparently, of instructions from headquarters to make the enquiries more thorough. Further, M. Legoyt (Chief of the Statistical Department for France, quoted by George Darwin), at about the same date, came to the conclusion that the true percentage of first-cousin marriages for the whole of France was much higher than Boudin had supposed, viz., about 3%.

In 1875 George Darwin published a long and careful paper upon "Marriages of First Cousins in England and Their Effects." In this memoir the author made the important point that in England and Wales cousin marriages are probably much more frequent, relatively, in the aristocracy than in the general population, and least frequent in London. He estimated that 3.5% of aristocratic marriages were between first cousins, but only 1.5% of all London marriages. The paper was the outcome of great labor and care, but the exact figures of the cousin marriages were admittedly open to revision.

In 1908 Professor Karl Pearson made a limited contribution to the subject. He found that of 1,600 members of the medical profession no less than 4.5% had married first cousins, and that if the lesser degrees of consanguinity were included the total was 7.5%.

This 4.5% of first-cousin unions may be compared with Legoyt's estimate of about 3% for the whole of France, and George Darwin's 3.5% for the British aristocracy.

Pearson further, from an examination of the books of the hospital for sick children in London, found only 1.3% of cousin marriages of all degrees up to third cousins, recorded in the histories of 700 in-patients. This so far as it goes confirms George Darwin's conclusion that consanguineous marriage was relatively infrequent in London; but the family histories of these 700 hospital in-patients were probably far from complete in regard to the point and 1.3% is almost certainly too low.

DISCREPANCIES IN DATA

From these discrepant, and avowedly incomplete, materials it seems probable that a class or clan or caste influence operates in certain cases to produce a high proportion of consanguineous marriages, and therefore conclusions as to any effects of consanguinity, whether bad or good, drawn from a *mass* population would not necessarily apply to all the groups of which the population was composed. However this may be, one certainly finds that in some individual genealogies cousins often marry, and in others seldom or never.

As to the second question: Are the defects sometimes observed in the offspring of consanguineous parents due to the consanguinity as such or on the other hand to both parents being tainted?

In regard to the *de novo* origin of defects in children of cousin parentage we find Charles Darwin stating his belief as follows, after having devoted much attention to the subject: "I hope to show in a future work that consanguinity by itself counts for nothing, but acts solely from related organisms having a similar constitution, and having been exposed in most cases to similar conditions;" and a recent authority, Professor J. Arthur Thomson, of Aberdeen, considers that "the idea that there can be any objection to the marriage of two healthy cousins who happen to fall in love with one another is preposterous." Many similar, and also some, but I think a diminishing number of, opposing, opinions might be cited.

What then is the origin of the view, or at least the suspicion, held by many, that consanguineous unions are injurious as such?

Without going back to the very early history of marriage customs and prohibitions—a task I am not competent to undertake—it is I think enough to say that the early Christian Church appears to be chiefly responsible for the existing residue of prejudice against the marriage of cousins. The church put its ban upon consanguineous unions;

at first in connection with the cult of asceticism and celibacy, later because it was able by the sale of indulgences to make money by allowing consanguineous couples to break the Canonical rules for a consideration. That this was so is confirmed by the subsequent extension of the prohibitions to various affinities, or even accidental associations, between persons not related at all by blood.

Thus—to quote from Huth's "Marriage of Near Kin" (p. 122), (1887)—the council of Trent in the middle of the sixteenth century issued the monstrous declaration "that the person baptized, his *parents*, *god-parents*, and the *priest* who baptized him were as much inter-related as though they were relatives by blood to each other," so that, as the author continues, "no tolerably near relative of the priest could marry either the godrelations or relations of any child that priest had baptized;" and there is much more to the same effect. Without enquiring too closely as to whether such absurd regulations were always carried out we may, I think, safely agree with Mr. Huth when he says that "the prohibited degrees were far too useful to abolish."

GENETIC CONSEQUENCES

Of course other causes have been and are still at work in both encouraging and discouraging consanguineous marriages. Any such influences as may possibly depend upon supposed social inconveniences or inexpediencies arising from these marriages are outside my purview. I think the most operative cause of such hostility to these unions as still exists is the confusion, already referred to, between *inheritance* of a defect from two *slightly* tainted, but apparently normal parents and the supposed creation of an entirely new thing by union between those of related blood. Such confusion is only too natural, and all of us have, I daresay, fallen into the pit at times—certainly I have done so formerly. For instance, if amongst the children of a pair of seemingly normal cousins there should be some born deaf and dumb the calamity cannot be hidden from the friends, and, as casual enquiries about cause are seldom carried further back than the parental generation, as most of us have a fatal facility for stopping at the first plausible excuse, no surprise need be felt if the cousinship, as such, is blamed; although had enquiry been possible or been permitted, cases of the same malady would very likely have been discovered in ancestors or collaterals.

That consanguinity of parents repeated through many generations is compatible with the maintenance of a high standard of health and vigor (mental and bodily) is demonstrated by well-known instances.

Near the mouth of the river Loire, on the Atlantic coast of France, is the small Commune of Batz, situated on a peninsula that is almost shut off from the mainland by a salt-marsh, so that the inhabitants have (or had prior to 1864 when the investigation now referred to was made) very little communication with the people of the mainland. The principal occupation is salt-making; and the inhabitants live an extremely simple life and crime is almost unknown. They have intermarried amongst themselves for, as it is put, "countless" generations. In 1864 M. Voisin, interested in the effects of consanguineous marriage, spent a month in personally examining the facts on the spot. Amongst the total population of the Commune, at that date 3,300, he found forty-six marriages that he counted as consanguineous. He made no attempt to tabulate the marriages of very distant cousins but states that the great majority of the marriages were of that kind, *i. e.*, he found but few married couples who were not related by blood in some degree.

COUSIN-MARRIAGES FERTILE

To begin with, the fertility of the forty-six marriages detailed as consanguineous was decidedly higher than the average fertility for the whole of France. Next, he failed to find amongst the entire population (3,300) a single case of any of the various diseases and defects that are always named as being supposed to result from consanguinity

of parentage, and he describes the people themselves as healthy, robust and intelligent. The death-rate of those who grew up was very low and many of them lived to a great age. The infant mortality, however, was very high, chiefly from acute diseases of the chest and throat and especially "croup." It is of course possible that amongst these young children who succumbed to acute infantile diseases there were some who would have shown inferiorities or defects had they lived; but even if we grant that, it is difficult to believe that not one of the survivors would have suffered in some corresponding way. Nor does it seem likely that the acute diseases of childhood would have selected a specially large proportion of those who might afterwards have shown degeneracies.

A very similar account was given in 1885 of the small fishing village of Staithes between Whitby and Saltburn. And quite a number of almost identical cases are upon record.

Of course plenty of examples are to be found where an excessive proportion of diseased and degenerate *are* found amongst the offspring of cousin-parents. But these prove no more than that if such degeneracies exist in the stock they may be transmitted.

That inbreeding, very much closer in degree and repeated far more often than anything in modern human society, does not necessarily lead to degeneracy but quite the contrary is shown by the history of modern breeds of domestic animals. For it is of course admitted, not only that the marvelous improvements effected during the last 150 years in the breeds of horses, oxen, sheep and pigs—to name only the more important kinds of live stock—have been reached by careful selection of the individuals possessing the characters desired; but that, as we are constantly told, the only way to secure and to fix such desirable characters is to carry out this crossing of near relations; *i. e.*, we are told that the desirable characters come as the result of crossing parents both of whom possess them in some degree. Doubtless the same parents sometimes also contain the rudiments of undesirable characters and degeneracies also, but such individuals will as far as possible not be used for breeding, and the production of the weaknesses they show will thus to a large extent be checked. It may therefore be asserted that the history of modern breeds of domesticated animals affords little, if any, support to the doctrine that marriages of blood-relations can produce qualities —good or bad—that are not represented at all in either parent.

CLOSE IN-BREEDING

There does, however, seem reason to believe that fertility is, or may be, diminished by *very* close in-breeding, repeated for several generations (Darwin, Animals and Plants under Domestication, II., 101, etc.); I mean, *e. g.*, by mating, say, brother (*a*) with sister (*b*), and subsequently the father (*a*) with their daughter (*d*), and again with her daughter (*e*) and so on or *vice versa* as to sex. It is further asserted that when such infertility has reached a dangerous degree it can often be counteracted, *i. e.*, the normal fertility of the race or species be restored, by crossing with a non-related stock; or what appears still more strange, by crossing with a very distantly related member of the same stock,— one derived from another branch of the same stem but perhaps reared under somewhat different conditions. We may perhaps take comfort from this and— whilst fully admitting that what is true for some of the lower animals may not always be true for man, especially in regard to the higher and distinctively human attributes,—conclude provisionally that if it should ever be shown that human cousin marriages were less prolific than others, this defect would most likely be neutralized by the next out-marriage.

Huth ("The Marriage of Near Kin," pp. 193-96), summing up the material then available (1887) as to the number of children born to parents who were blood-relations and to those that were unrelated, respectively, found that the consanguineous marriages appeared to be more fertile than the others; and although he thinks that, owing to uncon-

scious selection on the part of some of the authors whose data he quotes, the statistics are not entirely trustworthy, the conclusion is that consanguineous unions have certainly not proved to be less productive than others, and that the probabilities point to their being more so.

The only contribution I can make at the moment to this subject shows very little difference between the number of children born to parents who were, and were not, cousins respectively. The data relate to the chronic progressive eye-disease, Retinitis pigmentosa; the numbers are not nearly large enough for final conclusions, and the information on which they are based was sometimes unavoidably incomplete. But I give them for what they are worth. In ninety-three completed families of children (childships) the offspring of non-consanguineous parentage, containing cases of the disease just mentioned, there were 591 children, or 6.3 to each marriage (average). In forty-eight childships the offspring of consanguineous parentage (usually first cousinship) there were 259 children or an average of 5.5 to each marriage. The difference of fertility, such as it is, is against the cousin marriages, but as already stated we could not safely draw conclusions from such a small series, even if it were certain that consanguinity had been recorded in every instance where it had been present.

I think, therefore, we may conclude that marriages between cousins are as safe from the eugenic point of view as any other marriages, provided the parents and stock are sound.

The difficulty, of course, both for consanguineous and out-marriages is to decide upon this vital point; and as for obvious reasons the family history is more likely to be forthcoming for a pair of cousins than for an unrelated pair, we have here a part explanation of the aversion to cousin marriage met with in some families. This explanation will tell with special force if the disease or defect is relatively rare for then it will be more likely to occur, though in a latent form, in two cousins than in two strangers. But if the defect apprehended be a frequent one, *e. g.*, tuberculosis, the chances of the hereditary liability to it being present in both parents and intensified in their children may be much the same whether the parents were cousins or not.

It seems to me that since we as yet know next to nothing of how the various transmissible characters, good, bad or indifferent, are, or at least may be, linked together in inheritance; and that there are many other factors in marriage beyond those relating directly to race improvement or the reverse; it is best, in the present state of our information, not to discourage marriage between cousins unless there be a clear case; *e. g.*, inferiority or instability of a definite kind, or the history in the stock of such distinct diseases, or liabilities to them, as the eye disease, Retinitis pigmentosa, or deaf-mutism and others, more familiar to us, that could be named.

Pan-American Scientific Congress

The second Pan-American Scientific Congress will be held in Washington from December 27, 1915, to January 8, 1916. Eugenics has been alloted a place in the section for Anthropology, while plant-breeding and animal-breeding will come under the Section on Conservation of Natural Resources, Agriculture, Irrigation and Forestry. It is hoped that the principal genetists of Central and South America will be in attendance, and will describe the practical work in breeding which is being done in those countries. George M. Rommel, secretary of this Association, is chairman of the subcommittee dealing with conservation and agriculture.

GREEN LEAF IN CHERRY BLOSSOM

DAVID FAIRCHILD.

IT HAS long been known that the reproductive organs of a flower could, under certain circumstances, be replaced by leaves—either the ordinary stem-leaf or the flower leaf (petal). Double flowers are in most cases due to a replacement of stamens or pistils by petals, and are for this reason sterile. Examination of a carnation or almost any highly developed double flower will show that it must necessarily be sterile, since it altogether lacks the reproductive organs.

The appearance of an ordinary leaf in place of some part of the seed-producing apparatus is rarer, but an excellent example is shown in FIG. 7, a blossom from a Japanese flowering cherry (*Prunus pseudo-cerasus*) at my home near Chevy Chase, Maryland. The pistil is lacking, and its place is occupied by two well-formed green leaves (a) and (b), the teeth or serrations along the margins of the leaves being clearly visible. Such a phenomenon is technically known as phyllody.

The older botanists looked on such a change as a metamorphosis, believing that the pistil had actually been changed into a leaf. At present, it is more usual to regard it merely as a replacement, since this does not involve the assumption, formerly made without hesitation, that the reproductive organs are morphologically nothing but modified leaves, which might easily reappear through "reversion." The sporophylls or reproductive organs of the flower may be modified leaves, but it is equally possible that leaves are modified sporophylls; and until evidence is available from which the case can be proved one way or the other, it is safer not to assume that the sudden appearance of a little leaf in the center of the flower, as shown in the illustration, is a reversion of some organ to its ancestral type.

Even as to the evolutionary origin of the petals, we are not yet certain. A. P. de Candolle (1817) seems to have been the father of the idea which prevailed for nearly a century, that all the floral leaves are derived from sporophylls, the plant having found it presumably advantageous to modify some of its sporophylls in a conspicuous way, in order that it might attract the attention of insects and secure the pollination of the remaining sporophylls. Recently, however, Goebel and others have contended that the petals may in some cases be modified bracts or true leaves.

If we go back one step farther, we can disregard these slight difficulties and say broadly that a flower is merely a highly modified stem of the bud type. This will be recognized by anyone who examines a flower bud before it opens. The showy petals, which to the layman are the distinctive part of a flower, are in fact merely ornaments of comparatively late addition, from an evolutionary point of view, and not at all necessary to make a flower. From the morphological viewpoint the presence of the sporophylls, the organs which bear ovule and pollen grains, is the criterion recognized by most botanists. The origin of these sporophylls, the stamens and carpels, is obscure. They are very ancient structures, and although they are represented in lower plants by leaves bearing sporangia, or organs for the distribution of spores, it is possible, as was indicated above, that at a still earlier period leaves were the luxury and sporophylls of some kind the primary necessity of the plant.

The causes leading to such a replacement as that illustrated in the photograph are still almost wholly obscure. They are rather vaguely ascribed, in many cases, to an excess in nutrition, but there is reason to believe that in a large number of instances such a change is to be considered as originating in the germ-plasm. Evidently, a phenomenon of this sort is likely to complicate our idea of heredity in a very embarrassing way, and to cause us to hesitate before accepting too confidently any theory which will make heredity a simple matter of the shuffling of unalterable unit characters in a purely mechanical manner.

GREEN LEAF IN A CHERRY BLOSSOM

In this blossom of a Japanese flowering cherry (much enlarged) the pistil has been replaced by two green leaves (*a*, *b*). The pistil is the organ which is usually described as the female part of the flower; it includes an ovule, and some appliance for catching pollen grains and allowing them to fertilize the ovule. It is possible that it is, from an evolutionary point of view, a modified leaf: if so, such a phenomenon as is here shown would be a case of reversion. It is also possible that the leaf is a modified pistil. Photograph by Fairchild. (Fig. 7.)

PRACTICAL DOG BREEDING

A First Attempt To Apply the Modern Principles of Genetics to the Needs of Dog Fanciers.

REVIEW OF A BOOK BY WILLIAMS HAYNES

IT HAS long and deservedly been a matter of reproach to the science of genetics, that it has not succeeded in getting in sufficiently close touch with the practical, producing breeders of the country. Genetics is obviously one of the subjects, a knowledge of which would be of direct profit to almost everyone; yet the average man, even the average breeder, knows little of modern discoveries in heredity. The organizers of the American Genetic Association twelve years ago saw the need and attempted to meet it, believing that if there were some central body to collect and disseminate genetic information, plenty of individuals would be found to put it into further circulation. So far, this belief has hardly been justified, and the genetist must therefore welcome with particular warmth such a handbook[1] as the one which Williams Haynes, of New York, a breeder and judge of long experience, has prepared for fellow fanciers of the dog. The principal dissatisfaction likely to arise in the mind of a reader of it is that such handbooks are not available to devotees of every other kind of livestock.

DOG BREEDING NOT DIFFICULT.

Mr. Haynes has been remarkably successful in giving clear and simple expression to the ideas of genetics—translating them, as he says, into "dog talk" and usually illustrating his points with examples drawn from the kennel. While the book is thus specialized, as it should be, it yet deals with the fundamental principles of breeding so soundly that breeders of any kind of live stock would derive enjoyment and profit from reading it.

As is pointed out in the preface, dog breeders, like breeders of other kinds of pet stock, have lagged behind the breeders of stock for profit, and it is still not rare to find a dog breeder of note who will acknowledge that his system consists of "putting two good ones together and trusting to luck." Yet, "compared with other breeders, the dog fancier has an easy task. In the first place, he has less for which to breed. Secondly, dog histories and dog pedigrees have been for generations carefully recorded. Lastly, dogs have been bred toward approximately the same ideal for a considerable length of time." In spite of this, it is well known that dog breeding in the United States is in an unsatisfactory condition, and that fanciers are continually resorting to the importation of dogs from England or the continent. This condition could easily be remedied, Mr. Haynes thinks, if fanciers would study their breeds carefully and familiarize themselves with the value of the pedigree; and to emphasize the desirability of the latter end his handbook is largely devoted.

A chapter briefly recounting the facts of reproduction, from a biological viewpoint, is followed by one on variation, "the backbone of breeding." That with which the dog fancier is concerned is largely of a discontinuous nature, although it seems probable that some of the fancy points of highly artificial breeds appeared as discontinuous variations, or mutations. The screw tail of the English Bulldog is a case in point; it not only breeds true, but has been bred into the Boston Terrier and French Bulldog. The alleged production of variation through telegony and maternal impression is mentioned with proper

[1] Practical Dog Breeding, by Williams Haynes. Outing Handbooks, No. 30. Small 8vo, pp. 211, price 70 cents. Outing Publishing Company, 141 W. 36th Street, New York, 1915.

skepticism. As to the effect of climate, what Mr. Haynes says will be of general interest:

"The instances of deterioration in British breeds of dogs introduced into India have been often quoted from Darwin, but since he collected his information additional facts have been brought forward. A strong dog fancy has developed in India, with numerous shows under the jurisdiction of the Indian Kennel Club. Judging from descriptions and photographs, Indian breeders have been able to produce dogs that compare favorably with their direct importations from England, and today we hear little about the degeneration of dogs in the eastern country. Moreover, the success of the Airedale Terrier in the Philippines and throughout tropical America is further confirmation of the fact that climate does not have such a direct bearing on variation as was formerly supposed."

MENDELISM IN DOGS.

In his chapter of heredity, Mr. Haynes falls back largely on Galton, emphasizing particularly the law of regression. Mendelism is briefly described, with the comment that not enough experimental breeding has been done with dogs to throw much light on the unit characters present. The work of C. C. Little[2] makes it probable that the inheritance of coat color in pointers follows Mendelian rules, and can be bred for with confidence. "Dr. C. G. Darling believes eye coloring in Airedale Terriers is Mendelian, the light color being dominant. He acknowledges that he has not sufficient data to either prove or discredit this hypothesis, but, as an eye specialist and a terrier breeder, his opinion bears weight. If he is correct, it is probable that all eye color in dogs follows Mendelian inheritance." The same, as every one knows, holds true in Man, with the exception that here the dark pigment is dominant.

"It is also probable that the smooth and broken coats in Fox Terriers, a form of cross-breeding that is common, is Mendelian, the broken coat being in this case dominant. The red and black coloring in Chow Chows and self-colored spaniels is also probably according to Mendelian inheritance."

"In our conceptions of heredity," Mr. Haynes concludes, "we dog breeders have made two mistakes. These are natural ones, and it is some consolation to know that other breeders, and even trained biologists, have fallen into the same errors. In the first place, we have paid too much attention to the exceptional individual, the dog that is a 'stormer,' way above the average of his race. Secondly, and this sounds somewhat paradoxical, we have not paid enough attention to the individual points that go to make up the whole dog.

"In our almost fetish worship of the Champion of Record, we have been led astray in formulating any sound systems of breeding. We have overlooked the great average of the race and the drag that this average always exerts." And "although as breeders we are continually working for the development or effacement of certain points, we have overlooked the fact that these different characters behave differently in transmission. Some blend, others never do. Some are correlated, others quite independent."

SCIENTIFIC SELECTION.

These facts are the basis for the work of selection. The dog breeder is merely a spectator of variation and heredity. He can not control them directly; he must arrive at his goal by selecting from the material at hand. Mr. Haynes discusses selection under three heads.

(1) What is the true object of selection? "It is very evident that the only way the breeder can make any important, permanent headway is to bring the average of his own strain closer to the ideal expressed in the Standard than it is to the average of the race. In this way, and only in this way, can the drag of the race be lessened, and this drag is the breeders' worst enemy. Until he can overcome it, his breeding can only be partly successful. To overcome it, by raising the average of his own strain, is the true object of all selection."

[2] JOURNAL OF HEREDITY, V, 6, pp. 244–248, June, 1914.

Just what this drag of the race means to the breeder is expressed in mathematical form by Galton's Law of Ancestral Heredity, which is generally found to be substantially correct wherever large numbers are concerned, although it, like all other statistical generalizations, may be wholly misleading in individual cases. Galton's Law states the average contribution of each ancestor, all the way to infinity: in the following table only the first six generations are considered, the effective heritage contributed by each of these generations, and by each individual in the generation, being expressed in percents.:

Generation	Number of Ancestors	Influence of Generation	Influence of Individual
1	2	50.	25.
2	4	25.	6.25
3	8	12.5	1.56
4	16	6.25	0.39
5	32	3.125	0.10
6	64	1.5625	0.024

In other words, the individual gets only half his heritage from his parents, the rest from his more remote ancestors. The true object of selection, therefore, is to establish a strain in which the ancestors as far back as possible will conform to the desired type.

(2) Can selection accomplish this object? Theoretically yes, without any hesitation. "A somewhat involved mathematical proof has been worked out to show that after six generations of careful and continued selection a certain character will invariably breed true. No further selection for that point is necessary, provided no dogs which will deteriorate the inheritance for that point are introduced into the strain." A good pedigree for six generations is for most purposes as good as one for sixty.

"The breeder can expect that intelligent, continued selection will change type in any desired direction, and that new type will breed true after six generations of continued selection. He cannot, however, expect to accomplish any material reduction in the amount of variation. So far as the opportunity that variation always presents for further selection is concerned, the breeder will always have material available, but there are mechanical and physiological limits beyond which no amount of selection can ever be carried. However, in all probability, these limits have not been reached, except possibly in the size of the very large and very small breeds."

(3) What are the principles involved in rational selection? First and foremost, the breeder must know the points of the dog he is breeding. He must know the history of his breed. A knowledge of the famous dogs of the past will soon serve to locate prepotent strains, which have perhaps been produced by inbreeding, and which can be of great value. Study of pedigrees, then, is a matter of vital importance; and as a practical rule, the breeder must set before him an ideal and always breed toward it.

So much for the principles of dog breeding. In the second part of the book Mr. Haynes discusses practice rather than theory. What he says about inbreeding will be particularly interesting:

WHAT INBREEDING MEANS.

"The whole subject has been badly muddled by a loose use of the term inbreeding, and by very hazy notions on the part of every one concerned as to just how common true inbreeding is among dogs.

"Inbreeding means nothing more nor less than the crossing of the blood of one individual. There are only three possible ways in which this can be accomplished. (1) By breeding a sire to his own daughter. (2) By breeding

a dam to her own son. (3) By breeding together full brother and full sister. These, and only these, are true inbreeding." Any other consanguineous matings are to be called line-breeding.

As a fact, inbreeding is not common, it appears, among first-class dogs. An examination of records showed that "including both inbreeding and the primary cross of line-breeding only 7% of the Scottish terriers are closely bred, and but 13% of the Airedales." Mr. Haynes strongly indorses line-breeding, but holds that "continued inbreeding results in degeneration of both physical and mental powers."

As to breeding systems, whose name is Legion, Mr. Haynes reduces them all to six different basic systems, three of which "are no systems at all," and he takes occasion to warn his readers that successful breeding is not a matter of some mathematical or mechanical "system" but of knowledge and judgment. The systems he recognizes are:

(1) Trust to luck.

(2) Breed to the latest sensational winner. This he calls the "fashionable breeding system."

(3) The egotistical system—i. e., the one in which the fancier always breeds to his own dogs, "because it is cheaper, because it gives them greater opportunity as sires, or because it supplies the puppies with pedigrees that look as if he had established a strain of his own." These three systems are all actively bad.

(4) Inbreeding.

(5) Line-breeding.

(6) Outbreeding. Each one of these has its own place, but must not be considered as an infallible rule.

The effect of inbreeding is to magnify the heredity of a single individual. This is often highly desirable, and most breeds of live-stock have been built up by inbreeding. But as bad as well as good points are intensified, inbreeding is a two-edged sword which must be used with great care.

VALUE OF LINE-BREEDING.

"Judged by the results produced, however, line-breeding, although its results have been slower and are less sensational, has been even more effective. In our pedigree studies we saw[3] that line-breeding has, in the case of two typical terriers, produced more than five times as many champions as inbreeding."

"Line-breeding can be defined as the combination of the blood of a certain individual without the direct use of that same individual. It is fairly represented in the marriage of cousins in whose children the blood of the grand-parents is again combined."

"Straight out-breeding, the scrupulous avoidance of all close-breeding of any type, is a child of the super-fear of the noxious effects of continued close-breeding. From what has been said, it is plain that this is foolish and unprofitable. Moreover, from a practical point of view, it is almost impossible."

From these methods, Mr. Haynes "pieces together" a system that he feels confident in recommending to breeders. The breeder should learn all he can of the history of his variety and then he will draw up for himself a very definite ideal. His actual breeding operations will be directed toward the establishment of a strain that will as closely as possible approximate this ideal.

How can this be done? By transforming "the drag of the race" from an enemy to an ally, Mr. Haynes believes.

"Even a casual study of any breed will reveal the fact that certain points 'come good' in the majority of the dogs. Other points are commonly bad. If in the selection of the brood bitches of his kennels, a breeder gets two or three of sound average type, but excelling particularly in those points in which their breed, as a breed, is weak, he will have made the best possible start toward the establishment of that ideal strain. Naturally, these bitches should not only excel in these weak points of their breed, but should, so much as possible, be bred from stock strong in these same characters. Bred to dogs excelling in these same points, and better in others, the foundation of the strain is well laid.

[3] JOURNAL OF HEREDITY, V, 8, pp. 368-369, August, 1914.

"In every subsequent mating that takes place a breeder should always strive to hold every good point possessed by his bitch, and to add to them something extra from the stud dog. The dangerous pitfall that trips hundreds of thoughtful breeders is the attempt to balance points, good and bad, against each other. A bitch excelling in eyes, skull and ears but lamentably bad in foreface will be bred to a dog with capital foreface, but shocking in eyes, ears and skull. Or a bitch with speed and hunting sense, but lacking in bottom, will be mated to a solid dog with substance to spare, but little else to recommend him. As an example of glorious optimism such matings are splendid, but as breeding operations they are pathetic. The result is more than apt to be a spoiling of whatever good points were possessed by both parents, for these points were probably above the average of the race, and the principle of regression would tend to pull the avearge of the puppies back closer to the breed mean. Always hold then whatever good points we have, endeavoring in each successive mating to add to these other good points."

"Working on this basis, there are two things useful in narrowing the selection of the individual dogs in any particular mating. Always judge a dog as a breeding unit not by its own points, but on its ancestry and progeny."

"The elimination of all guess-work and the willingness to accept considerable length of time before success comes should be the first resolution made by a breeder. This means study, first, of the principles of genetics; next, of the breed with which one is dealing; and lastly, of the individuals employed in every mating. Working upon a foundation of excellence in the weak points of his breed and always retaining good points gained and adding others to these, the breeder, provided he judges his breeding stock by their puppies rather than by their own points, is sure to establish a strain upon which he can count for results. This, however, can not be done in a season. The breeder must possess those qualities we all admire in our dogs, patience, gameness and faithfulness."

Fruit Breeding in Alaska

There are two species of strawberry wild in Alaska, one in the coast region and one in the interior. Pollen from both of these has been used in hybridizing cultivated varieties of the strawberry, in order to induce hardiness. The crosses with the native plant of the coast region have been a conspicuous success. Nearly 4,000 of the hybrids thus produced have fruited and some hundreds of them are good enough to be retained for further testing.

The crosses with the native plant of the interior have been but few, and so far without noted improvement on the cultivated parent, except in matter of hardiness.

We have crossed the cultivated raspberry with pollen from the native salmon berry and produced many seedlings. The hybrids, however, are nearly all sterile; nothing of value has been produced, but we propose to continue crossing with other native species of the genus.

Attempts are now being made to cross native species of Ribes on cultivated varieties of *Ribes rumbrum* and *R. nigrum*. Some work is also being done with gooseberries.

We have for several years attempted to cross the native crabapple on cultivated varieties of the apple, but for various reasons little seed has been produced. The work is continued, however. It is almost certain that if apples are ever to be grown successfully in Alaska, the trees must contain the blood of this native crabapple. Imported cultivated varieties, even the hardiest and earliest of them, can be grown only with partial success.

C. C. Georgeson.
Special Agent in Charge of Alaska Investigations, U. S. Dept. of Agriculture.

DETERMINATION OF SEX

Experimental Biology Making Progress in the Solution of Fundamental Problem—Sex-control in Men Now Impossible but May Some Day Be Realized.

REVIEW OF A BOOK BY LEONARD DONCASTER
Fellow of King's College, Cambridge, England

HARDLY any problem in biology has aroused such interest as that of sex, and the amount written on it is staggering—the number of hypotheses put forward to explain the determination of sex is said to be not far from 500. As long as the problem was attacked only by wordy speculation, based on philosophical theories or observation of uncontrolled phenomena, little progress could be made; but the experimental school of biology attacked it in a more direct way, and is beginning, its adherents think, to see light. "The last few years have seen a considerable advance, and we now know at least something of the causes which lead to the production of one or the other sex, although of the manner in which these causes act our ignorance is still profound."

The experiments of the biologists are scattered through a voluminous periodical literature, and it is therefore well worth while to have them brought together and digested by a man who is himself a leader in the work. Dr. Leonard Doncaster, superintendent of the museum of zoology at Cambridge University, has undertaken the task, and his book,[1] "The Determination of Sex," is not only up-to-date and critical, but written in a clear and pleasing style, which makes it a welcome addition to genetic literature. At a time when biologists tend to express their ideas in mathematical symbols, there is need for leaders of the science who are able to expose their subjects in literary English; and Dr. Doncaster proves himself to be such a one.

His review is also marked by an unusually conservative attitude. "The study of sex has not yet reached a stage at which it is possible to give an account of the established facts, and of generally accepted inferences from them, which shall be even comparatively free from controversial matter," he remarks in his preface. "The subject has been approached by many quite different lines, and these lines, although convergent, have as yet given no indisputable indication of the central point towards which they all tend." Nevertheless, some solid facts seem well established, and it may be of interest to review these in company with Dr. Doncaster. His book does not discuss sex-determination in plants, and a large part of his illustrative material is drawn from breeding experiments with insects—work to which he himself has for a number of years given a large part of his time.

THE PURPOSE OF SEX

In the first place, the author does not subscribe to any one of the theories which have been confidently put forward to explain the origin of sex—the reason why the existence of two sexes is an advantage to a species. "It is a remarkable thing," he observes, "that apart from the fundamental attributes of living matter—assimilation, irritability, growth, and so forth—no single character is so widely distributed as sex; it occurs in some form in every large group of plants and animals, from the highest to the lowest, and yet of its true nature and meaning we have hardly a suspicion. Other widely dis-

[1] The Determination of Sex, by L. Doncaster, Sc. D. Pp. xii+172, 8vo, 22 plates; glossary and bibliography. Cambridge (England), University Press; New York, G. P. Putnam's Sons; 1914. Price, $2 net.

tributed characters have obvious functions; of the real function of sex we know nothing, and in the rare cases where it seems to have disappeared, the organism thrives to all appearance just as well without it. And in many other cases, especially in plants, where sex is definitely present, it may apparently be almost or quite functionless, as, for example, in the considerable number of plants which are habitually grown from grafts or cuttings, and in which fertile seeds are never set. It is of course impossible to say with confidence that such 'asexual' reproduction can go on quite indefinitely, but the evidence formerly adduced that continued vegetative reproduction leads to degeneration has been shown to be of doubtful validity. Sex, therefore, although it is almost universally found, cannot be said with certainty to be a necessary attribute of living things, and its real nature remains an apparently impenetrable mystery."

To understand the efforts made to solve the problem, it is necessary to have a clear appreciation of what sex is, and of the mechanism of sexual reproduction. We speak of certain kinds of animals and plants as female, and of other kinds as male: "The fundamental thing about the female sex is that female individuals produce bodies known as egg-cells or ova, which after uniting with a cell of a different character derived from the male, develop into new individuals." This description fits equally an animal or a plant. "Superficially, egg-cells vary greatly in appearance; they may be relatively large, owing to the inclusion of nourishing substance or yolk for the developing embryo, or they may be microscopic, as they generally are when no yolk is present. They may have a special protective covering, or may be naked, but apart from these differences, which are so to speak accidental, they are always characterized, in the most various animals and plants, by consisting of a mass of relatively unmodified protoplasm[2] containing a single nucleus.

"As the distinguishing character of the female is the production of eggs or ova, so that of the male is the production of male germ-cells, which, however, vary greatly in different cases. They are characterized by the fact that their function is to reach an ovum and unite with it in the process of fertilization, as will be described in more detail below. In nearly all animals and in many of the lower plants, the male germ-cells are for this purpose endowed with the power of independent locomotion; in animals they are called spermatozoa (in the singular spermatozoon, sometimes abbreviated to sperm) and in the lower plants spermatozoids. In the flowering plants the male germ-cells are enclosed in the pollen grains which are produced by the stamens of the flower; the pollen-grains have no power of independent movement, but are carried to the neighborhood of the egg-cell in the female flower, or part of the flower, either by wind or by insects, and thence grow out a tube which penetrates to the egg-cell and carries the male germ-cell into contact with it. Although, therefore, there are great differences between the male germ-cells of different organisms, they all agree in one essential feature, they are adapted for reaching in some way the more stationary egg-cell, they unite with it in the process of fertilization, and the *zygote* so produced proceeds to develop into a new individual. In animals, in which the spermatozoa have the power of independent movement in a fluid, these are commonly more or less tadpole-shaped, consisting chiefly of a 'head,' which contains little else beside the nucleus of the cell from which they have been derived, and a vibratile protoplasmic tail by the motion of which they travel through the fluid in search of the egg-cell.

"The essential feature of the process of fertilization is the union of the two nuclei contained respectively in the egg-cell and the head of the spermatozoon. The nucleus is a portion of protoplasm enclosed in a thin membrane

[2] "Protoplasm is the name given to the substance which is the material basis of all living things. In chemical constitution it resembles white-of-egg, and consists of very complex compounds of carbon, hydrogen, oxygen and nitrogen, with a smaller amount of sulphur, phosphorus and other mineral elements."

and differing from ordinary protoplasm in containing a quantity of a substance called *chromatin*, so called because it takes up stains (Greek, *chroma*) more readily than other parts of the cell. In its ordinary condition the chromatin is scattered in fine granules on protoplasmic threads enclosed in the nuclear membrane. We know that the nucleus is of fundamental importance to the life of the cell; metaphorically we may say that the nucleus is to the cell what the brain is to the body. When a spermatozoon meets an egg cell, it forces its way into it, until its head is embedded in the egg protoplasm; the tail is then dropped off, and the head, which consists almost entirely of a very concentrated nucleus, swells up until it reaches the size of the nucleus of the egg-cell. The two nuclei then slowly approach each other until they meet, when they fuse into a single zygote-nucleus, which immediately begins to divide in such a way that equal parts of both parental nuclei are contained in each half. The whole cell then divides in two, and the process is repeated until an embryo containing thousands of cells is produced. In every division the nucleus is divided in such a way that both paternal and maternal portions are accurately halved; from this it results that every cell of the offspring contains equal parts of the paternal and maternal nuclear substance (chromatin)." It is by the study of the details of this process of division that more light has been thrown on the problem of sex-determination, perhaps, than by any other method.

MIXTURE OF INHERITANCE

"In the process of fertilization we get almost our only definite indication of the ultimate nature and function of sex. We have seen that every part of every individual includes equal portions of nuclear matter descended from one and the other parent. The mechanism for producing this equal division is extremely beautiful and complex, and it is impossible to believe that it is not of fundamental importance. There is evidence that the nucleus, and particularly its chromatin, is especially concerned in the transmission of inherited characters, and the mechanism of nuclear division insures that, of this 'material basis of heredity,' the portion derived from each parent shall be equally distributed to every part of the body. One of the chief effects, then, of sexual reproduction, and perhaps its most important function, is the equal mingling in every individual of sets of inherited characters derived from two parents."

But such a result, whatever its advantages to the organism and the species may be, might also occur in the absence of two distinct sexes. "If all individuals produced similar germ-cells, and if these united with one another at random, we should still get fertilization and a similar recombination of characters, although there would be no division into males and females." Such a condition is, in fact, found in some of the simplest one-celled animals and plants, such as Protozoa, where two similar individuals fuse to form one. "In the higher, multicellular animals, such union of similar cells is never found, and even in the Protozoa it is usual that the two cells which conjugate (*i. e.*, unite in fertilization) should be dissimilar. One cause for this is not difficult to discover. The zygote formed by conjugation of two germ-cells has to grow up directly into a new individual, and if the function of fertilization referred to above is to be fulfilled, the germ-cells must come from different parents. If both cells were alike, it would be much more difficult for them to meet each other than if one is relatively large and stationary, the other small and active. Further, it is a great advantage to the embryo that it should be provided with some source of nourishment in its early stages. This is done in many cases by the storing up of food-material (yolk) in the egg, and it would clearly be impossible for two such yolk-laden eggs to seek each other out and unite. In a number of animals belonging to various groups, and also in the flowering plants, the embryo is supplied with nourishment direct from the mother, and this again would be impossible if both germ-cells

were alike. Hence it becomes clear that if once it is admitted that sexual reproduction is necessary or advantageous to the organism, the distinction of the two sexes, male and female, follows almost inevitably."

DIFFERENCES BETWEEN SEXES

Although this distinction between male and female as sperm-producer and egg-producer is the fundamental one for our present purpose, there are in general great differences of other kinds between the two sexes. The female in most species is relatively quiescent, passive, and devotes her time to storing up energy, for the benefit of her young; the male is "vigorous, restless and active, and characterized by the dissipation of energy rather than the storing of it." These physiological differences lead to the production of striking external differences, seen in the more conspicuous adornment of the males of most species, and the relatively inconspicuous appearance of most females; they even extend to mental characteristics of human beings, "since woman is said to be more receptive and conservative, while men show greater originality and are more inclined towards change."

In the adult animal, then, there is usually no difficulty in telling at a glance to which sex an individual belongs; "even in species in which the sexes are externally similar an examination of the mature reproductive organs will always reveal the sex with certainty." But in the embryo this is generally not the case: at an early, or sometimes even a late stage, it is impossible for the observer to say whether the individual is going to become a male or female. This puts sharply before us the question on which students have always been divided: whether at such an early stage the sex is already determined, or whether it is still to be determined by some environmental condition or conditions. In other words, is the sex determined when the egg and sperm cells unite, or is the embryo then and for some time afterward indifferent in character, and able to become either male or female, according to conditions? The latter theory has always been the more popular one, but the Mendelian school of geneticists, and many of those who have studied the cell with the microscope, during recent years, incline to believe that the sex is fixed, once for all, when the ovum and spermatozoon come together. Each side has strong evidence and, as Dr. Doncaster points out, it seems very unlikely that both can be right; he therefore attempts to find some sort of a reconciliation between them.

PARTHENOGENESIS

First we must consider cases in which the sex is determined before the egg is laid. Such is the case among those insects, for example, where the reproduction is parthenogenetic—where the egg is never fertilized by a sperm. In such cases, it can often be predicted with certainty whether a given egg will result in a male or female. To quote a single instance, the plant-louse Phylloxera, which has so ravaged the vineyards of Europe, produces two kinds of eggs—large and small. Neither kind is fertilized by a male, but the former always produce females, the latter males. Among the Rotifers, and elsewhere, an egg which would have yielded a male if it had developed without fertilization, produces a female if it unites with a spermatozoon. The case of the honey-bee is well known: queens and workers are produced from fertilized, drones from unfertilized eggs. "All the eggs are as far as can be discovered originally alike; all undergo a similar maturation process, and the sex of the offspring seems to depend entirely on whether the egg-nucleus conjugates with a sperm-nucleus, or develops unfertilized." In this case, external conditions cannot determine sex. Among many similar cases, that of identical twins may be cited, as observed to good advantage in man and the armadillo. When twins, quadruplets, etc., are produced by the division of a single egg, rather than by the fertilization of several separate eggs, the individuals are always of the same sex, indicating that the sex must already be fixed immediately after the

fertilization of the egg, since division of the early embryo always gives rise to individuals of like sex.

The complicated phenomenon of sex-limited (or sex-linked) inheritance, which has been carefully investigated by many genetists during the last fifteen years, throws further light on the problem of sex-determination. Color-blindness in man is a common example: affected men married to normal women have, as a rule, only normal children; their sons show no tendency to transmit the affection, but some of their sisters and probably all of their daughters may transmit it to their male children. It is thus commonly said that the defect *appears* in men and *is transmitted by* women. It has been possible to determine exactly the proportion of affected individuals likely to appear in a given family; but in many cases these proportions are not found exactly, even in breeding experiments where large numbers are dealt with. The evidence from sex-limited inheritance can not be reviewed at length here, but it is of such a nature that to Dr. Doncaster "it seems impossible to doubt that sex-determining factors are borne by the ova and spermatozoa, and from the regularity of the observed results, that sex is in general fixed from the moment of fertilization and is not altered by events which may take place later." On the other hand, the occasional upsetting of the expected ratio indicates that other, obscure causes may be sometimes at work.

EVIDENCE FROM CELL-DIVISION

The next line of attack which Dr. Doncaster considers is one that has been pursued for only a few years, but which has proved particularly profitable—namely, the study, microscopically, of the germ-cells at the time of their divisions. The evidence here obtainable is based on the behavior of those much-talked-about bodies, the chromosomes.

"The nucleus of any cell in its ordinary condition is enclosed in a membrane, and consists of a more solid substance bathed in a fluid. Scattered evenly over the network are exceedingly minute granules of the substance known as chromatin which is especially characteristic of the nucleus as distinguished from the cell-protoplasm. When the nucleus is about to divide the chromatin gradually collects into masses, probably by the contraction and concentration of the threads of the network. These chromatin masses are known as *chromosomes*, and in general both their number and their relative sizes are constant not only in all the cells of any individual, but in all the individuals of any species Their number varies greatly in differen species, so that the chromosome number may be regarded as a definite specific character." At each cell division, the chromosomes divide in halves, one half of each going into each one of the daughter cells. The number of chromosomes in the daughter cells is therefore the same as that in the mother cell, being in each case the number characteristic of the species. But if a sperm or egg-cell is preparing for conjugation, it goes through a so-called "maturation division" at which the number of chromosomes is reduced to one-half that characterizing the species. When it unites with a cell of the opposite sex, the full complement is thus restored. Obviously, if it were not for such a maturation division, the number of chromosomes in the fertilized egg-cell would be doubled at each generation. Without stopping to discuss the complex details of this cell division, or the action of the chromosomes in parthenogenesis, let us proceed at once to the most interesting part of the subject—the now famous case of the *X-chromosome*.

In some species, one sex has one less chromosome than the other, and this odd chromosome is known as the X-chromosome. The other sex, possessing an even number, is considered to have two X-chromosomes. In the white human race, for example, von Winiwarter found forty-seven chromosomes in the male, the forty-seventh being the X-chromosome, while the female seemed to have forty-eight, both the forty-seventh and forty-eighth

being assumed to be X-chromosomes.[3] Now the X-chromosome is conspicuous because of its unpaired condition, in contrast with all the rest of the chromosomes in the cell, and it can therefore be followed under the microscope, by anyone possessed of good eyes and unlimited patience. Its behavior has, in the minds of many, gone far to settle the question of sex-determination.

THE UNPAIRED CHROMOSOME

"It was known at the close of the last century," Dr. Doncaster reminds us, "that in insects of the order Orthoptera (grasshoppers, etc.) one chromosome behaved differently from the others in the development of the spermatozoa, and it was soon found that this chromosome was unpaired, and that in consequence half of the spermatozoa possessed it and half were without it. Its difference in behavior consists in its remaining as a compact body while the other chromosomes have the form of elongated loops at the stage at which the pairing of the ordinary chromosomes takes place, and it is this difference in behavior, in addition to its apparent connection with sex-determination, that justifies its designation by a special symbol as the X-chromosome. After the unpaired X-chromosome had been discovered in the males of certain Orthoptera and Hemiptera (plant-bugs), it was found that a pair of such chromosomes was present in the females of the same species. Since, therefore, in these forms, the female before the maturation divisions has two X-chromosomes and the male only one, it follows that after maturation all eggs possess an X-chromosome, while half the spermatoza have it and half do not. The eggs which are fertilized by spermatozoa containing X will then give rise to individuals which have two X-chromosomes, and will become females, while those fertilized by spermatozoa without X will develop into individuals with only one X, and these will be males."

If men of science could have stopped here, we might think that the problem of sex was a simple one. But it was soon found that although the condition above described existed in a number of species, conditions quite different were met with in other species. Both sexes have two X-chromosomes, in many species; but in many of these it was later found that one of the X-chromosomes in the male was smaller than, or different from, the other. It was accordingly named the Y-chromosome; and it was decided that, by the laws of chance, all fertilized eggs would have either the constitution XX or the constitution XY: the former would clearly become females and the latter males. But it has been pointed out above that the conditions in some species, particularly insects, show that there must be two kinds of eggs, some male-producing and the others female-producing. The "XY hypothesis," therefore, which would make the determination of sex wholly a function of the spermatozoon, acting under the law of probability, could not be supposed to have an absolutely universal application. Further investigation has brought many complications to light, so that sex is not now considered by most students to be universally and absolutely dependent on an X-chromosome, although there is a good deal of evidence to show that this unpaired or unequally-paired chromosome has an improtant rôle in the transmission of sex-limited characters.

IMPORTANCE OF CHROMOSOMES

The simplicity of the "X-hypothesis" is so attractive that it still has many adherents, but "it cannot be regarded as proved. Many eminent biologists believe that the chromosome behavior is not the cause, but is, so to speak, a symptom of sex, and that the cause of sex-determination lies deeper. No one believes that the presence of horns is the cause of a Red Deer being a stag instead of a hind; horns are a regular accompaniment of maleness in most deer, but are certainly not its cause. So it may be maintained that an egg

[3] In the negro race, Guyer's observations show a double X-chromosome in the male, with a total of twenty-two chromosomes. The count of large numbers of such small and elusive bodies is extraordinarily difficult, and results are not to be accepted too confidently.

or a spermatozoon may have one chromosome more or less because it has in it the power of developing into one or the other sex, rather than that the chromosome is the cause of sex. In any case, it is highly probable that sex is not determined simply and immediately by the presence or absence of a chromosome. Even if the chromosome behavior is a necessary link in the chain of causes leading to sex-determination, as seems to the writer probable, it is not the immediate or the only cause of sex."

If the determination of sex were due to such a simple cause as the law of chance, the two sexes ought to be produced in equal numbers, unless disturbing causes interfere. Everyone knows that in many animals, at least, the sex-ratio is approximately 1:1, as this hypothesis demands; but when the statistics are large enough, it is in general found that the excess of males is greater than the hypothesis in its simplest form calls for. Worse, the sex-ratio seems to fluctuate according to a variety of conditions.

"For 100 females born, it has been found that in man the ratio of males averages between 103 and 107, in the rat about 105, in the horse ninety-eight, in the dog about 118, rising in some breeds to over 140, while some animals have an even greater divergence from equality." The ratio is still more upset in man when still-births are counted, for a large part of early births and abortions are those of males, who apparently have a lower vitality than females at that stage of life. Evidently, then, the proportion of young of the two sexes produced is not equal, as the chromosome hypothesis seems to demand.

It has not been difficult, however, to find hypotheses to explain this. Two, at least, can be supported by some facts: "If it is assumed that the spermatozoon determines sex, the male-producing spermatozoa may perhaps be somewhat more active, or for some other reason, such as slightly smaller size, more successful in entering the egg; if, on the other hand, the egg has some share in sex-determination, there may be a tendency for the polar divisions to occur rather more often in such a way as to produce eggs of one sex rather than the other. Because, therefore, the sexes are not exactly equal in number the hypothesis of sex-determination by the germ-cells which unite to form the fertilized egg is not disproved. What is required is to investigate the causes which have been found by observation to influence the sex-ratio, and to relate them, if possible, with chromosome behavior."

EXTERNAL INFLUENCES

Such influences are to be found in temperature, nutrition, age of one or both of the parents, moisture content of the germ-cells, age of germ-cells at time of fertilization, hybridity, etc. The experiments and observations on the influence of these various factors are numerous, and most of them treacherous. In many cases they can be shown to have no effect on the sex-determination ratio, but merely to produce a differential mortality in the sexes. In other cases, the determination of sex does seem to Dr. Doncaster really to have been influenced. "On the whole, therefore, the study of the sex-ratio, while not leading to any positive conclusion with regard to sex-determination, makes it necessary to reconsider the simple hypothesis of final determination by one or other of two kinds of germ-cells, to which the facts of sex-limited inheritance and of chromosome behavior seem naturally to lead."

Lack of space prevents a review of the interesting chapters on secondary sexual characters, hermaphroditism and gynandromorphism. Dr. Doncaster's general conclusions must be considered at some length, even though the reader of this review has not been given in many cases the evidence on which they are based.

"Beginning with the question of the stage at which sex is determined, it was shown that in some cases it appears to be determined already in the unfertilized egg, in other cases to depend on the spermatozoon and to be fixed at fertilization, and in other cases again to be capable of modification

during the embryonic development or even at a later stage.

"Evidence for determination by the egg apart from fertilization was drawn (1) from the facts of parthenogenesis, (2) from sex-limited transmission by the female, and (3) from cases in which two kinds of fertilizable eggs are produced, which differ from each other in their chromosomes. In all these cases it is certain that, normally at least, male-determining and female-determining eggs are produced, and that if the kind of egg is known, the sex can be predicted without reference to the spermatozoon.

"Similarly, evidence for sex-determination by the spermatozoon is provided (1) by the cases in which unfertilized eggs yield males, fertilized eggs females, as in the bee; (2) by sex-limited transmission by the male, and (3) by the existence of two kinds of spermatozoa differing in respect of their chromosomes. In each of these three groups the evidence for sex-determination is exactly comparable with the similar determination by the egg in the previous class."

A SIMPLE HYPOTHESIS

If this were the extent of our knowledge, we would have fairly clear sailing. "Sex might be regarded as depending on the presence of a greater or less amount of some 'sex-determining substance' present in the chromosomes, or more correctly, on the physiological condition arising from the interaction of this substance with the substance of the cells. The presence of an additional 'dose' of this substance in a cell otherwise similar would alter its metabolism (*i. e.*, general physiological condition), and since all cells of the body would contain the extra dose, the whole physiology of the body would be affected, and the sex of the animal would be irrevocably determined."

"So far the problem is relatively simple: although nothing is known of the manner of action of the sex-determining factor supposed to reside in the sex-chromosomes, it can at least be said that in the cases mentioned it is inherited like any other Mendelian character (as was first suggested by Bateson and by Castle), and that individuals which receive it from both parents would be of one sex, those to which it is transmitted by one parent only, of the other sex.

"To this scheme, so attractive in its comparative simplicity and its close accord with the facts on which it is based, there are opposed a series of observations, usually derived from special cases and differing widely in kind among themselves, any one of which might perhaps be regarded as due to error or to chance, but which, when taken together, make a rather formidable obstacle to the acceptance of the hypothesis. They may be grouped under two heads, including (1) evidence that the egg may influence the sex in cases in which observations on chromosomes indicate that the sex should be determined by the spermatozoon; and (2) evidence that the sex may be modified after fertilization by influences acting on the embryo or even later in life."

These various objections are of such weight, in Dr. Doncaster's mind, as to make him feel certain "that sex-determination does not depend on an unmodifiable unit, but rather on the reciprocal action between an inherited factor and its surroundings." In the absence of disturbing factors, sex will indeed be determined by the chromosomes; and "if the difference between the chromosomes of the male and female is considerable, it will outweigh any other influences which might tend to affect the general result." If the difference is not great, the chromosomes can absolutely determine the sex only under favorable conditions; if other agencies intervene "it becomes possible for an egg which would otherwise have been female to develop into a male."

SEX-DETERMINATION IN MAN

Finally, a few words on the ever-interesting subject of sex-determination in man. From what has been said, it will be fairly obvious that we are not now in a position even to predict the sex of any child, much less to control it. But the possibility of eventual control

does not seem to Dr. Doncaster to be absolutely lacking. Certainly none of the means so far suggested, some of which have attained considerable notoriety, has any scientific standing, but this does not prove that we may not some time know more than we do now.

On the basis of Dr. Doncaster's hypothesis, it is obvious that control of sex might be possible in one of two ways: either by seeing that the child received the proper chromosomes at conception, or by finding some influence which would act on the embryo after conception so as to modify the determination of the chromosomes, if it were not what was desired. It is hardly possible to conceive of a way in which sex might be controlled by the first means, and a solution is therefore probably to be sought along the second line suggested.

While the male in the human species carries the X-chromosome, and therefore should determine the sex of the offspring, there is some evidence to indicate that the egg-cell also has an influence in determining the sex. So far, attempts to take advantage of this—*e. g.*, by special diet for the mother—have met with no success. The idea that the right ovary produces male eggs and the left one female-producing eggs also rests on a very slender foundation. When the huge mass of evidence available is carefully criticized, it becomes evident that at present we have absolutely no knowledge as to how any condition can so affect the mother as to determine the sex of her offspring; nevertheless in the light of Dr. Doncaster's hypothesis "it would follow that the search for means of influencing the sex of the offspring through the mother is not of necessity doomed to failure." It cannot be too strongly emphasized that at present there is no means known to science of controlling sex in the human species, but it seems to the author still reasonable "to retain an open mind and to regard the control of sex in man as an achievement not entirely impossible of realization."

Bud Selection in Apples

One of the important projects in hand at the Illinois agricultural experiment station concerns bud selection with apples. C. J. Crandall writes:

"Under this project the effort is being made to ascertain whether there are differences in value between buds taken from different portions of trees for purposes of propagation. Buds were selected for testing this in three different ways: first, on the basis of size; second, on the basis of the location on the tree; and third, on the basis of location on the shoot or branch. All selected buds were measured and described and then grafted on ordinary apple seedlings. The trees grown from these buds have been planted in orchard form, and there are now living 1,395 trees. Annual growth notes are being kept up on all trees and it is proposed to grow them to fruiting age before attempting to report on their behavior. I can only say at the present stage of the project that there appear to be some slight differences in favor of buds of large size, and in favor of terminal as contrasted with lateral buds."

Hardy Peaches for Missouri

Hardy peaches are being bred at the Missouri experiment station by crosses between large fruited, purple twigged sorts including Elberta, Champion, Carman, and the green twig sorts represented by Rise's Seedling and Snow. The work with peaches has shown them to be self-fertile; therefore growers who plant only one variety need not fear that they will not get a crop, although such planting with apples, pears and many other fruits, largely self-sterile, would be disastrous.

DEVELOPMENT OF A CHERRY

DEVELOPMENT of a fruit from a flower is a process the details of which vary widely in different species of plants, but of which the general outlines are fairly constant. In general, it may be said that the internal changes are very complicated but the external changes quite simple. The process can be easily followed in the accompanying illustrations, made from Japanese flowercherries (*Prunus pseudo-cerasus*) and Japanese apricots (*Prunus mumme*) and considerably enlarged.

Starting with the flower, as shown in Figure 8, we notice first of all the beautiful pink petals. These represent the expenditure of an immense amount of energy on the part of the tree, and yet have no direct connection with the production of seeds—which may be said to be the tree's main purpose in life. When the ground below the tree is wholly carpeted with fallen petals, it may look as if the amount of energy thus used was wasted, but as a matter of fact the petals have served their purpose, and nearly every one knows that their purpose is a very useful one—that of attracting insects to the flowers, and thus securing cross-pollination of the blossoms, in order that two lines of ancestry may be united in the seed, and the resulting plant thus benefit from the traits of two parents. After the petals have fulfilled their end in attracting bees to the flower, they are of no further use to the tree, and are dropped.

The other conspicuous parts of the flower are the stamens or pollen-bearing organs, and the style, an organ designed to catch pollen carried either by wind or insects and transmit it to the ovule lying in the ovary below. It is not necessary here to follow out the details of this process of pollination—most readers understand how the pollen grain sends a long tube into the style and down to the ovary, and how the cell-nuclei of the pollen grain slip down this tube and unite with the ovule. There are then two pairs of cell-nuclei; one pair unites to form the embryo, and the other to form what is known as the endosperm—the starchy material surrounding the actual germ.

This union of the male and female cells—what the genetists call zygosis—not only starts a complicated series of cell divisions in the ovule, leading to the production of the seed, but also stimulates the remaining parts of the flower to renewed growth. The stamens, whose function is finished after they have shed their pollen, wither and drop off in time. The style has also finished its work after it has furnished a route for pollen grains to reach the ovule, and it accordingly begins to wither; but as it is firmly attached, being in reality a modified prolongation of the end of the stem, it remains in place for a long while, frequently until the fruit has reached considerable size, as shown in the left of Figure 9. Finally, however, with the increasing size of the fruit, a layer of abscission cells is formed at the base of the style, all its nutrition is cut off, and the wind some day blows it away.

We have thus disposed of all the parts of the flower—petals, stamens and style—which were most conspicuous when it first appeared. Meantime, a part that was then apparently of little importance has developed remarkably. This is the ovary, which is from a historical point of view a thickening of the stem to act as a receptacle for the ovule—which, when fecundated, becomes the seed. As the seed develops, so does the ovary enclosing it. The changes here are principally chemical in nature, and not yet wholly understood. The tissues of the ovary swell up and become distended and succulent. Part of the starch is changed into sugar; considerable quantities of acid are formed, coloring matter is deposited all through the tissues, and finally, aromatic substances are created. This by no means catalogues all of the complex chemical changes, but it suggests

JAPANESE APRICOT BLOSSOMS

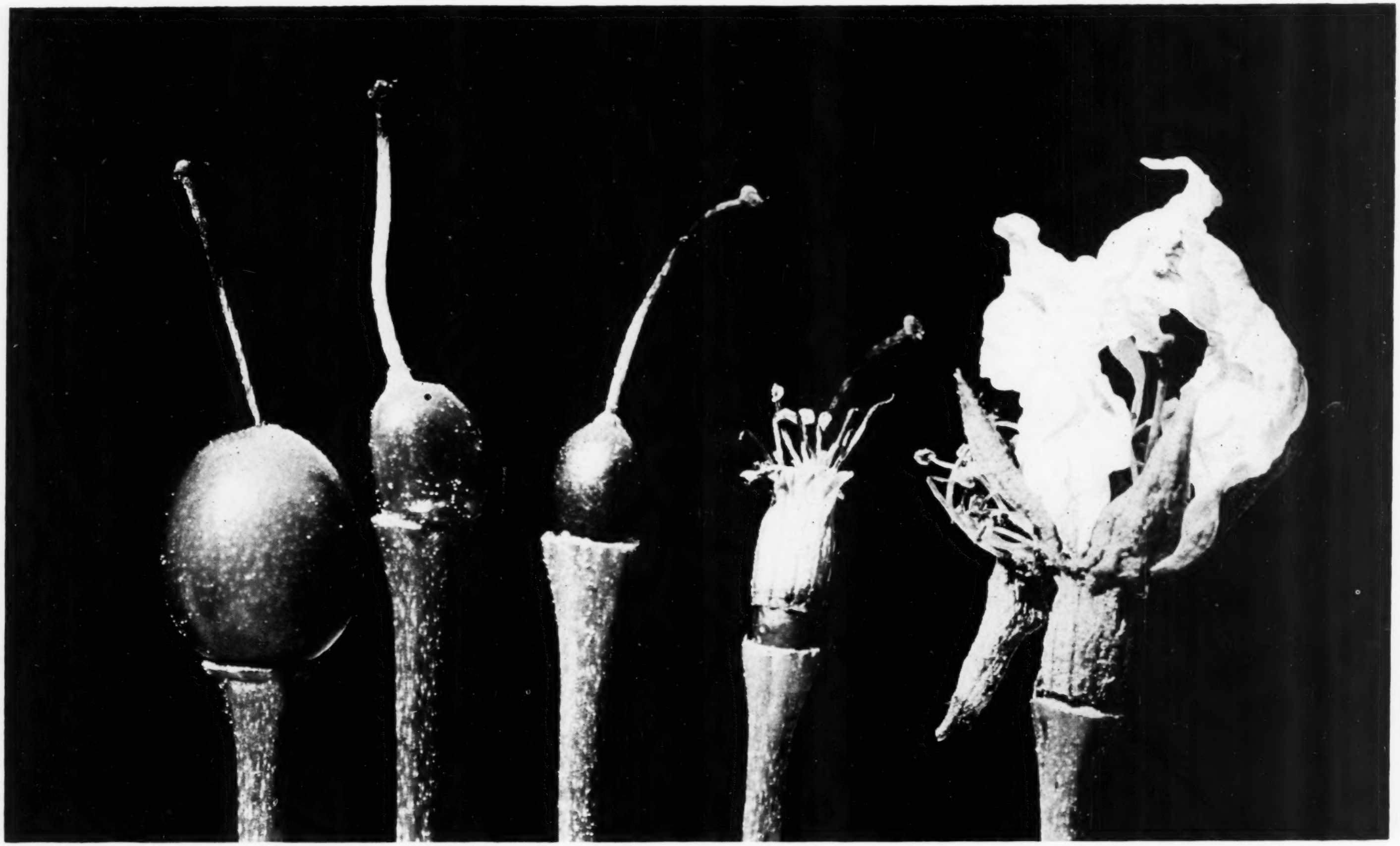
The conspicuous parts of the blossoms—petals and stamens—have no direct part in the formation of the fruit that is eaten, the function of the stamens being to furnish pollen, and that of the petals to advertise the existence of nectar, and attract bees who will distribute the pollen. Photograph by Fairchild. (Fig. 8.)

the most important ones. By the time the seed is fully mature and capable of germination when placed in a suitable environment, the ovary surrounding it has become the bulky structure which we know as the cherry.

Presumably the advantage to the tree in thus surrounding its hard seed with a succulent mass of flesh is that birds are led to eat it, and thus carry it to considerable distances. The seed coat is hard enough so that it passes through the intestinal tract of the bird without damage, and is finally excreted to lodge in some new locality and take root. The devices which Nature has developed for seed dispersal are varied, and some of them are, to man, of a disagreeable character. It is, to say the least, fortunate that some of them on the other hand are of such an agreeable character as our edible fruits.

Mendelism in Melons

Mendelian inheritance in melons has been worked out to some extent at the New Hampshire agricultural experiment station, through crosses of muskmelons with canteloupes. In the hybrid generation blending was found to be the rule, but the following generation revealed the segregation of six pairs of contrasted characters, or allelomorphs. Advantage can be taken of these results in scientific breeding of melons.



DEVELOPMENT OF A CHERRY

First, as shown on the right, the petals wither and fall, then the stamens and calyx likewise disappear. The style frequently adheres until the fruit has reached a considerable size. The details of the development of the fruit are described in the text. Photograph by Fairchild. (Fig. 9.)

PUTTING OVER EUGENICS

Making It a Living Force Depends on Sound Application of Psychology and Sociology—Camp Fire Girls an Organization Which Will Create Eugenic Ideals in Women in an Indirect but Effective Way

A. E. Hamilton.
Extension Department, Eugenics Record Office, Spring Harbor, Long Island, N. Y.

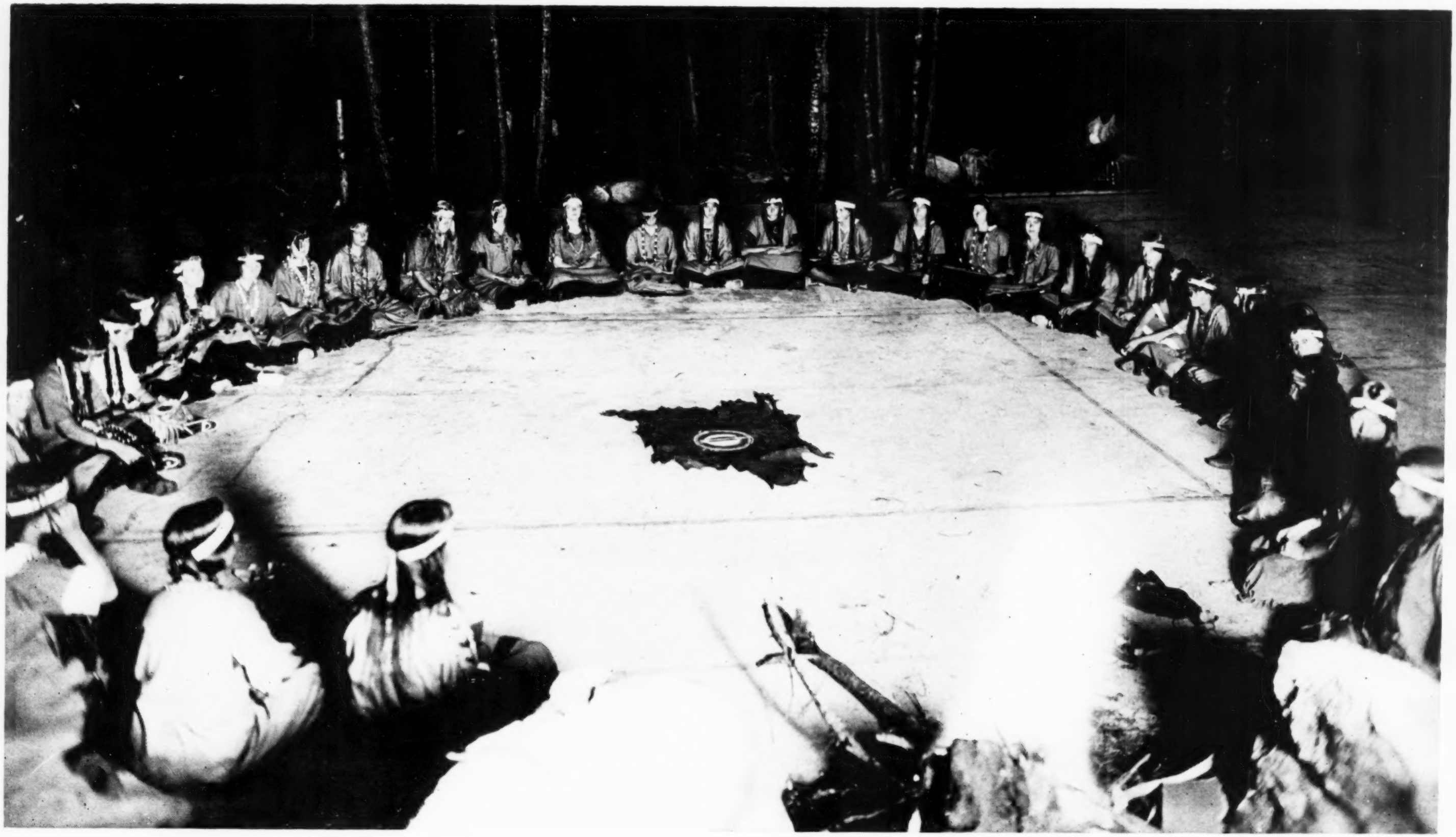
ONE of the finest women, physically, mentally, spiritually, that I have been honored to meet in my lecture travels told me that she and her sisters had refrained from marriage because her family tree showed branches withered by tuberculosis.

One of the brainest and brawniest young men I have ever known told me recently that the process of trying to rationalize his emotions and bring them into line with his intellectual content regarding fitness for marriage had led him to leave the side of the woman who would have made a splendid wife had they married, and betake himself to the solitary ways of scientific research.

One of the most sympathetic and child-loving men who has crossed my path writes of breaking an engagement on the score of finding doubtfully dysgenic influences in the family of his sweetheart, of falling in love with another admirable young woman and then tearing himself out again because of something he deemed not quite fitting in her hereditary constitution, and of finding another girl who seemed quite eugenically built, but who turned him down gently but flatly when he suggested that they be married for the sake of wellbornness in children.

And I might go on recording more of this sicklying o'er with the pale cast of bluegenic thought the young minds of potential parents, which seems to me almost the net result of many of the well-intended but myopic endeavors hitherto made to put over Galton's idea to the coming generation. Perhaps it is time for those of us who have the welfare of Eugenics at heart to ask ourselves what there is in a monograph on albinism, a study of the inheritance of idiocy, or a lecture on the declining birth-rate in college women or American men of science, to promote a desire on the part of our fittest young people for marriage and parenthood—for if that be not the keynote of Eugenics, then I sadly misconstrue the meaning of the word.

To which it may be answered that all our normal young folks desire marriage and parenthood—whereupon the question becomes; Do they desire it enough to overcome the obstacles, psychological, social and economic, that are thrust in their way because the conscious attention of those who should be educators and leaders has not been turned on these problems sufficiently to make a dent in them? Marriage and parenthood must ever remain matters of desire, and the work before the true promoter of Eugenics is that of social engineering which will make for the realization of desire in the wholesomest and quickest way. The man whose genius has given us the first self-supporting social institution designed for the most genuine race-betterment puts the challenge thus, "Can you develop the geniuses who will parallel in the social world what our inventors have done in the world of steam and electricity? Can you then create social self-supporting institutions that shall take these social inventions and so put the power of organization, publicity and finance behind them that they shall be as wide spread as the telephone, the movies or the telegraph?" In the organization of the Camp Fire Girls, the brain child of Doctor and Mrs. Luther Halsey Gulick, we have the pioneer type of such self-reliant and self-supporting institutions as will doubtless spring up



THE CAMP-FIRE CIRCLE

As far as is possible, Camp Fire girls are trained to act as hostesses in their communities. On such occasions as the one here pictured, they learn to seek the best side of the character of each individual, and to show that side to the individual and to others. What this may mean to the Eugenics movement is pointed out by Mr. Hamilton in the text. Photograph by Mrs. Luther H. Gulick. (Fig. 10.)

DEVELOPING THE BODY

Water sports and other carefully chosen forms of recreation are, in connection with the other Camp Fire activities, developing a spirit of physical and mental freedom in girls that will affect for good the parenthood of the next generation. Photograph by Mrs. Luther H. Gulick. (Fig. 11.)

to meet the demands of our new day. That here we have a splendid instrument for the promotion of true Eugenic ideals is sure if the words of its founder are made good, and from my own first-hand observations I should say that already they are being splendidly realized:

IDEAL OF THE FOUNDERS.

"Camp Fire Girls undertake in so far as they are able to act as Hostesses for their communities. The word Hostess brings to mind not so much one who is correct in matters of social usage as a woman who understands, who has insight, sympathy and tact. She often enables people to do what they never knew they had it in them to do. She reveals people even more to themselves than she helps them reveal themselves to others. She draws out each one's abilities by her power of intelligent appreciation. To her the arts of entertaining are those which bring people together, which reveal people to each other, which develop the social nature. To amuse is not her object, but to so treat her guests that the best in each is developed and fed by the best in everyone else. This is social genius. Camp Fire Girls aim to discover, develop and use social genius just as previous generations have discovered and used scientific genius. Into this work is being thrown an army of picked girls under the guidance of the ablest women. As they grow in power of team work, preserving the devotion and vision of youth, gaining

GIRLS MUST BE PHYSICALLY FIT

The Camp Fire is intended to educate girls in the true sense—that is, to give as full opportunity as possible to their valuable inborn characteristics to get expression. Physical fitness is one that, in the interests of the race, is naturally insisted upon. Photograph by Mrs. Luther H. Gulick. (Fig. 12.)

vision by experience, who shall say that they will not do for the affections as brilliantly as the previous generation has done for the industries. Both of these had their origin in the home but have grown beyond its confines."

"Camp Fire Girls is an organization of girls and women to develop the home spirit and make it dominate the entire community. Hence, the ranks should be recruited from those who have ability to do and to help rather than from those who need help. It is an army of girls rather than a mission to them. Military training is good for the health, but an army cannot be built out of invalids, selected because they need the outdoor life and exercise of a soldier. The purpose of Camp Fire is to find the ablest girls and women and to give them a training in team work that will enable and incline them effectively to give woman's service to the community. Girls and guardians do become improved by Camp Fire work just as the soldier is benefited by the army drill, but self improvement is no more the object for the girls than it is for the soldier. The primary purpose is to develop ideals, to train leaders, to create custom and fashion, habit and want. As Camp Fire girls grow up into women, they will have the training, experience and affection to help organize and carry on all of the social relations of the community, including the care of those who primarily need help. There are many institutions

"CAMP FIRE IS AN ARMY, NOT A HOSPITAL."

The organization is not intended primarily as a training school for the unfortunate, but as a training school for those who by birth and in other respects are likely to be leaders. It aims to help the mass of the population indirectly rather than directly, by turning out women who will be able and willing to disseminate its ideals in their own communities. Photograph by Mrs. Luther H. Gulick. (Fig. 13.)

GAINING CONTROL OF SELF

Fitness for motherhood is a happy by-product of Camp Fire activities, which make for splendid physique and intelligent control of one's own body and mind, and of Nature as we find her. Photograph by Mrs. Luther H. Gulick. (Fig. 14.)

of the hospital type, devoted to the unfortunate, and a few—if any—are working on this task to which we have devoted ourselves—that of building up an army of splendid women, trained in team work, devoted to the spiritual ideals of the home, and united in giving service to the community."

On being asked how Camp Fire girls were instructed and trained for motherhood, the mother out of whose home-camp life the movement originally sprang replied that no particular training or instruction was given to girls that could be labelled "for motherhood." The concept motherhood will not shrink into a course of training or into a text book. It is to be lived, not defined or imparted. Camp Fire girls win honor beads by work in seven crafts—health craft, camp craft, nature lore, home craft, hand craft, business and patriotism. As each girl receives honors she progresses from rank to rank in her group and in the organization, so that each attainment of an honor represents the realization of a desire for definite accomplishment. If the home craft honors lure girls to do those things that inevitably make for the best in home building and tending, if health craft and camp craft and Nature lore bring strong bodies, tested nerves and a realization of the mystery and purpose of love in the world of living things, then a big part of training for motherhood is accomplished as a by-product. But the girls have not been striving to become good mothers according to rule. They have been "seeking beauty," (which is the first of the seven laws of the Camp Fire) in a multitude of wisely selected and elective activities, and all the rest will have been added unto them.

EMBODIMENT OF EUGENICS

So, too, Eugenics, though nowhere mentioned in the literature of Camp Fire, finds its embodiment in the learning by doing which is Camp Fire. "To know the names, homes and occupations of grand-parents" for the earning of an honor in Patriotism, takes a girl into the fascinating field of genealogy where a little observation will suggest the segregation of hereditary characters, physical and mental, in the family history and so open up the study

A TEST OF SELF-RELIANCE

Only those who have tried paddling a birch-bark canoe in rough water and wind can appreciate the kinds of qualities that this girl is developing. Photograph by Mrs. Luther H. Gulick (Fig. 15.)

of inheritance in a natural, positive and interesting way.

To "investigate the effects of ventilation and sanitation in stores and factories employing women," another elective requirement for a bead, might lead to inquiries concerning the transmission of acquired characters, the difference between immediate effects of environment on women and their children and the strictly racial modifications of circumstance and time. The "study of ten public institutions in her locality" can hardly fail to acquaint a girl with the purpose and content of the poor-house, jail, insane asylum or home for the feeble in mind, and such a study, amateur and superficial as it may be, will bite deeper into the mind than the reading of a shelf full of official reports later on. A patriotism paper on Immigration brings up the great issues of value in human stock and the right to enter our gates. Studied and written about in a spirit of interest in subject matter plus the desire for accomplishment and the relation of the part to the whole, the Jukes and Ishmaelites and gunmen who flood into our country through the leaks in our immigration laws become real and living, and who that knows the slightest of the psychology of adolescence will doubt that such an interest, so stimulated, will influence the vote to be cast on these questions a few years later?

"To be familiar with National History as it affects woman's welfare" has already led two Camp Fire girls of my own acquaintance, walking in entirely different spheres, to hit upon the idea of camp fire and scouting activities as small beginnings of what William James dreamed might prove a moral equivalent of war.

Thus "the study of agencies under social control that may improve or impair the racial qualities of future generations either physically or mentally" is begun quite unconsciously, and

carried far beyond the academic stage of reading, writing and discussion, into the play-practice by a coming generation that will better fit it for facing these big world problems than perhaps any generation has ever been fitted before.

From fifty to seventy thousand young girls absorbing the Camp Fire environment, and their numbers increasing at the rate of about 2,000 girls a month, actually means something for the future of the race. The hiking, canoeing, swimming, cooking, and all-round glorification of work which seemed to my young friends a possible "moral equivalent of war," is developing a spirit of physical and mental freedom that will doubtless result in a finer fitness for motherhood and a keener perception of what is wanted in fatherhood for the next generation. It will also be found to be working out a solution for many an economic problem that results from our extravagance in luxuries, which are more than compensated for in the happy recreations of the out-of-doors and the simplifying of customs of eating and all-round living that comes with an appreciation of a life close to the heart of nature, whose wayward children we still are, despite our carapace of over-civilization.

THE TORCH-BEARERS

The highest rank in Camp Fire is that of the torch-bearer, whose desire, expressed on receiving the honor, is:

"The light that has been given to me
I desire to pass undimmed to others."

And its biological and social significance is made plain to those who work for and win the rank. In such a spirit we may hope to find at least the beginning of a realization of Galton's hope that Eugenics might become a living part of our very religious conception of life; for surely, as Saleeby has aptly put it: "If we have transferred our hopes of heaven to earth, and from ourselves to our children, they are no less religious, 'and they that shall be of us shall build up the old waste places; for we shall raise up the foundations of many generations.'"

Nor need this be conscious or reasoned. Perhaps the less we say to our young people about Eugenics directly, the better. Surely the less they have to deal first-hand with the black-symboled pedigrees of neuroses and disease that constitutes the major part of our present crop of eugenic literature, the more wholesome will be their attitude toward their own relation to life and its multiplication. It is not so much seeds of fear concerning going wrong that we need to sow, as it is seeds of strong desire to go right and pass on undimmed the light that is given us in a spirit of hope that the best is yet to be, for heresy though it seem to say so in this journal, it is our social heredity that preserves the best the race has wrought out of space and time, and the unmodifiable germplasm is, after all, more a matter of concern to the microscopist and biochemist than it is to our harvest of boys and girls. The hope I see for putting over Eugenics to the masses who need it most lies in the new interpretation of the world's poetry and romance, the making of life now and here romantic and interesting and splendidly worth while, so that we shall strenuously desire to perpetuate it and enjoy the holy fruits of its perpetuation, and this will be done only when we do as has been suggested, turn the current of humanity's genius into social channels and evolve as magnificent engines for social righteousness as we have for the discovery and control of weakness, disease and death. That this can and will be done we may already read in the signs of the times, not the least of these our Scouts and Camp Fire.